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Vol. XV, No. 4 *PSYCHOLOGICAL REVIEW PUBLICATIONS* AUGUST, 1932

AUG 1 1932

Journal of Experimental Psychology

18

EDITED BY
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PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter, July 1, 1920, at the post-office at Lancaster, Pa.

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

EDITED BY

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Review, Bulletin, J. Exp., and Index: $19.00 (Foreign, $19.75).

Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, NEW JERSEY

Journal of
Experimental Psychology

VOL. XV, No. 4 AUGUST, 1932

THE EFFECT OF RATE OF SEQUENCE UPON THE ACCURACY OF SYNCHRONIZATION

BY HERBERT WOODROW

University of Illinois

The primary purpose of this experiment was simply to determine the facts concerning the accuracy with which subjects can tap or 'beat time' in synchronization with a regular series of sounds and, in particular, to study the relationship between accuracy of synchronization and the rate of sequence of the sounds. The investigation was largely motivated, however, by the thought that it might afford new evidence, positive or negative, concerning the existence of a 'critical' interval in the neighborhood of six or eight tenths of a second, a matter which has been the object of much discussion and investigation in connection with the perception of short temporal intervals, and in general yield information connected more or less directly with problems in the psychology of time and rhythm.

The experiment consisted essentially in presenting to the subject a series of evenly spaced sounds of uniform loudness, termed the stimulus-series, and having him tap out a series of sounds, termed the series of tapped-sounds, so that the tapped-sounds would synchronize with the stimulus-sounds. The source of sound in the case of both series was the impact of the arm of a pony relay against one of the contacts of the instrument. Separate relays, of course, were used for the two series, the stimulus-series and the tapped-series. The stimulus-sound relay was actuated by an electric current which was made and broken at regular intervals by means of a platinum strip which brushed successively over a series of flat-surfaced 'contacts.' This brush was carried by the end of the rotating arm of a Meumann's 'time-sense' machine and the flat contacts were set upon the circular, graduated rim of this machine. The Meumann's machine as furnished by the manufacturer was dismembered and what in reality was a new machine was constructed by a mechanic who attached both the rim

and rotating arms of the Meumann's machine to a General Radio synchronous motor, geared so as to make one revolution in 4 sec when driven by a 60 cycle current. The revolving arm of the Meumann's machine was fastened directly to the shaft of the motor. The rate of sequence of the stimulus-sounds was controlled by the number of flat contacts placed on the rim of the machine. Since the speed of revolution of the rotating arm carrying the platinum strip was constant, it is obvious that the greater the number of contacts the faster would be the rate of sequence of the stimulus-sounds governed by these contacts. As a further check upon the rate of sequence and the exactness of the intervals, the interval between the sounds produced by each pair of successive contacts was repeatedly measured by means of a Hipp's chronoscope, controlled by a Klopsteg fall-apparatus.

The subject produced his sounds by tapping upon a Scripture's noiseless key so connected that an electrical contact which was made in the course of the downward movement of the finger closed a circuit actuating the arm of a pony-relay. The sounds due to the subject's taps thus remained always the same in loudness and quality and were independent of the force of his taps. To the subject, these sounds seemed to coincide exactly with the end of the downward movement of his finger. Graphic records of the stimulus-sound and movements of the key, alongside a 60 cycle per sec time-marker indicated that the stimulus-sound followed the contact which produced it by about 4 σ and followed the end of the downward movement of the key by about 2 σ. On account of the elasticity of the metal strip, on one end of which the subject tapped, the subject got no sensation of resistance at the moment that the other end of the strip closed the sound-producing circuit but felt resistance only at the end of the downward movement of his finger as it pressed the end of the strip on which he tapped into a thick layer of felt. In tapping noiselessly upon any surface it is always possible, particularly when tapping slowly, artificially to cause a displacement of the moment of apparent contact by exerting extra pressure after the first instant of contact and thus, by delaying the instant of *maximum* pressure, making the contact seem later than it really was. In short, from the sensations from the tapping finger it is not always possible to determine the precise point in time at which one should regard the tap as having occurred. The tap can best be given an instantaneous temporal location when it is accompanied by a sharp 'impact-sound' or click. The relay governed by the subject's taps was adjusted to produce a sound of the same loudness, judging by the ear on the basis of casual observations, as an exactly similar relay used as the source of the series of 'stimulus-sounds.' The sounds produced by the two relays were similar, but distinguishable, instantaneous 'clicks.' The discrepancy between the tapped-sounds and the stimulus-sounds, that is, the error of synchronization, was measured by means of the Hipp's chronoscope.

The subjects were instructed to attend to the two series of sounds and they clearly understood that the synchronization which they were to seek was the synchronization of the sounds constituting the stimulus-series and the sounds which they themselves produced, and not that between the stimulus-series and the end, or any other phase, of their finger-movements. The subjects were further instructed to think of the series as a series of discrete, separate sounds, all of which were evenly spaced and equally loud and to avoid any intentional grouping or subjective rhythm. Introspective reports were asked for only at the end of every second day. Emphasis was laid upon the fact that each tap counted in the data to be obtained and that the subject should therefore at all times do his best to synchronize. He was told further that if he felt he was not doing his best or became dissatisfied or confused he should stop at once and that the lost taps would be made up by extending the sitting.

Six subjects were used with each of eight rates, the rates varying from one sound in every ¼ sec to one sound in every 4 sec. The rates used, and the order in which they were used, were the same for all subjects. Only one rate was used on any one day. The various rates succeeded each other first in the order shortest to longest, and second, in the reverse order, from longest to shortest. That part of the experiment with the first order is termed the ascending series and that with the second the descending series. In the case of three of the rates additional series were also taken in order to study the effect of practice, but these practice series, which came at the end of the work, and the results of which will be given, are not regarded as part of the main experiment.

At each sitting the subject was given a test consisting of eight forty-second periods of synchronous tapping, with a rest of ½ min between each period. The first three of these periods were purely practice periods, and the subject was so informed. The practice periods were given every day throughout the entire work. No measurements were made during the practice periods, and therefore their effect is not known. They were given partly because it had been observed in some preliminary try-outs of the technique, with other subjects, that some subjects became confused or 'rattled' when first attempting to synchronize and partly because of the belief that in general test-results are more reliable if obtained after a fore-period of practice. It was during the last five forty-second periods, following the three practice periods, that all the measurements were made. Now in five forty-second periods the number of taps made by a subject would be 16 times as great at the ¼ sec rate as at the 4 sec rate, being 800 at the former and 50 at the latter rate. Since it is desirable to obtain the same number of measurements at each rate, it was decided to keep the total duration of time that a subject tapped at one sitting constant, but to get equal numbers of measures by making measurements of the subject's errors only in the case of one tap every 4 sec. This means that at the slowest rate the subject's error was measured at every tap but at the fastest rate only at every sixteenth tap. This method, which may be termed the sampling method, in addition to yielding equal numbers of measurements at all rates, probably affords more reliable or representative results at the faster rates than would be obtained by the continuous method, that is, by measurements of successive trials. The reason for this is that the sampling method, by representing a much longer performance than the continuous method (at the fastest rate here used, 16 times longer), includes responses which constitute samples of a larger number of the various attitudes that a subject tries out from time to time and is therefore less dependent upon any one attitude or small group of attitudes. On the other hand, the method of measuring successive reactions is the only one which permits the study of the 'drift' phenomenon, to which attention was called by Dunlap.[1] By the 'drift' phenomenon is meant the fact that often, after the reactor has adopted a rate of tapping that leads, more or less gradually, to noticeable asynchronism, *i.e.*, after he has drifted noticeably ahead or behind, he begins afresh at a somewhat different rate in the attempt to correct the discrepancy. Since the study of this phenomenon formed no part of the purpose of the present study, the technique of keeping the total duration of the trials constant, no matter what the rate, and measuring only one error every 4 sec, proved satisfactory. The subjects were unaware of the fact that not every error was measured.

The decision to employ chronoscopic in place of graphic technique entailed certain problems of set-up, since not only was it necessary to measure the interval between the

[1] K. Dunlap, Reactions to Rhythmic Stimuli with Attempt to Synchronize. *Psych. Rev.*, 17, 1910, p. 411.

stimulus-sound and the corresponding tapped-sound, irrespective of which came first, as is not particularly difficult, but to distinguish between the positive and negative errors, a distinction which the chronoscope itself can hardly be expected to make. Moreover, it seemed probable that the errors of synchronization might occasionally be zero and rather frequently amount to only a few sigma. The chronoscope is not adapted to the accurate measurement of such extremely short intervals. The difficulties were solved by inserting in the middle of the interval just before one of the contacts a momentary 'break' contact, the purpose of which was not to produce any sound but to cause the chronoscope's hands to start turning. These hands were brought to a stop as the result of the following sound. Thus, if the rate of sequence were one sound every 400 σ, this 'break' contact and the following sound-producing contact would be spaced so that the interval between the two as measured by the chronoscope would be in the neighborhood of 200 σ. This interval may be termed the control-interval. Its length was determined by 10 measurements taken immediately before and 10 more immediately after each sitting of the subject. The variation in this interval was so slight as to be negligible. A shift of a key changed the connections so that the chronoscope recorded the interval between the 'break' contact and the subject's tapped-sound, which interval may be termed the response-interval. Thus in connection with each sitting two intervals were measured, the 'control' interval and the 'response' interval. The former interval was that extending from the 'break' contact to the following stimulus-sound while the latter was that extending from the same 'break' to the following tapped-sound. Since the control interval remained constant throughout the sitting, it is obvious that if the subject synchronized perfectly, the response interval would equal the control interval. Any difference between the two intervals would represent an error of synchronization. It follows that the average error of synchronization, that is, the constant error, could be obtained by subtracting the obtained average length of the response interval from that of the control interval. For example, if the interval between the special 'break' contact and the following stimulus sound, that is, the control interval, was .192 sec and that between the same break contact and the following tapped-sound, that is, the response interval, was found to be .185 sec, the average error of synchronization would be −.007 sec (the negative sign indicating that on the average the subject tapped too soon).

The functioning of the apparatus may best be explained in detail by reference to the diagram (Fig. 1). The diagram omits the synchronous motor and the electric circuit passing through its driving magnets. The function of the motor was simply to cause the arms of the Meumann's machine, which were attached directly to its shaft, to rotate at uniform speed. The Klopsteg fall-apparatus is also omitted. It was connected so that its upper break corresponded to the break at S and its lower break to that occurring at B_2. It could probably have been dispensed with entirely without appreciably affecting the results. Sufficient control of the chronoscope, in view of the method used, was secured from the intervals controlled by the synchronous motor.

The central purpose of the apparatus may be stated as that of yielding comparable measurements of two intervals, which have been termed above 'control' and 'response' and which will now be more briefly designated (1) and (2). The discrepancy between these two intervals indicated the error of synchronization. Both intervals began as the result of a break in circuit X at S, produced by that end of the rotating arm of the Meumann's machine opposite the end carrying the platinum strip. The break in circuit X, a shunt around R_1, resulted in R_1 becoming effective and therefore in a break at B_1, thus causing the chronoscope hands to start running. It should be

pointed out that the break at S did not affect the sound-producing relays R_3 and R_4. Although both intervals (1) and (2) began as a result of the same break, one of them (1) ended with the stimulus-sound following this break while the other (2) ended with the corresponding tapped-sound. Which one of these two sounds would result in the stoppage of the chronoscope hands depended upon whether the switch K_3 was open

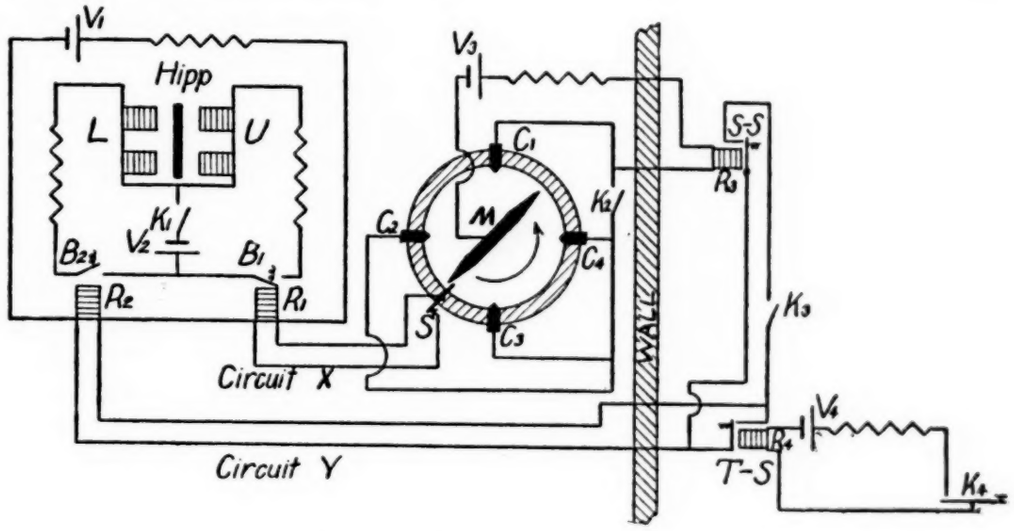

FIG. 1. Diagram of apparatus.

Hipp = Hipp's chronoscope.

U, L = upper and lower magnets of the chronoscope.

B_1, B_2 = two points in chronoscope circuit, at which breaks could be produced. A break at B_1 started the chronoscope, at B_2 stopped it.

Zigzag lines = adjustable resistances.

R_1, R_2, R_3, R_4 = pony relays (resistance 20 ohms).

M = rotating arm of Meumann's machine, one end of which carried a platinum strip which brushed over the contacts C_1, C_2, etc., and the other end of which hit and opened the break contact S.

C_1, C_2, C_3, C_4 = contacts placed on rim of Meumann's machine (the number of contacts used depended on the rate desired and varied from 1 to 16).

S = special break contact. As set in the diagram, S would be opened about halfway between the sounds resulting from the contacts at C_4 and C_1.

Circuit X = a circuit (shunting relay R_1) which when broken by the rotating arm of the Meumann's machine at the special break contact S, caused a break at B_1, thus starting chronoscope hands.

Circuit Y = a circuit which when made (by sound-producing impacts at either S-S or T-S) caused a break at B_2, thus stopping chronoscope hands.

S-S = stimulus-sound contact (made by arm of relay R_3).

T-S = tapped-sound contact (made by arm of relay R_4).

K_4 = tapping key (Scripture's noiseless) producing makes.

K_1, K_2, K_3 = switches (knife); K_1 and K_2 unessential; K_3 closed during controls, open during subject's tapping. When K_3 is closed, the stimulus-sound resulting from contact C_1 stops chronoscope hands; when open, that tapped-sound intended to synchronize with the sound produced by contact C_1 stops chronoscope hands.

V_1, V_2, V_3, V_4 = storage batteries.

or closed. When closed, the stimulus-sound ended the interval, when open, the tapped-sound ended it. K_3 was closed only before and after the subject's tapped-series; otherwise both the stimulus-sounds and the tapped-sounds would have ended the interval, and would thus have interfered with each other. This explains why the control-interval was measured only before and after the subject's sitting. The sounds terminating either interval (1) or (2) did so as the result of a 'make' of circuit Y, resulting from the impact producing the sound in question (a make at S-S in the case of (1) and at T-S in the case of (2). The closing of circuit Y resulted in stopping the chronoscope hands by actuating in the case of both (1) and (2) exactly the same mechanism, *i.e.*, short-circuiting or shunting relay R_2 and thus causing a break at B_2. Since the process by which both intervals were recorded by the chronoscope was identical, it follows that any constant error attaching to the measurement of either of them would be present to the same degree in the measurement of the other. Small errors in control of the chronoscope, or errors due to the latent periods of the relays, R_1 and R_2, etc., would therefore be of no consequence.

The Hipp's chronoscope was connected so that current ran through both the upper and lower magnets. The current through the upper magnets was slightly stronger than that through the lower, and the adjustment of the springs of the instrument was such that when no current was running through either the upper or lower magnets (K_1 open) the arm of the Hipp was nearer the upper than the lower magnets, *i.e.*, the hands of the instrument were in the stationary position. The hands of the Hipp, it will be remembered, run only when the arm is down near the lower magnets.

To understand in detail the action of circuit X, it should first be observed that the arm B_1 of relay R_1 was held closed by the spring attached to it. The spring served to close B_1 automatically after each momentary break at S. The circuit through the upper chronoscope magnets thus remained closed as long as R_1 was short-circuited by circuit X. When circuit X was broken, by a break at S, R_1 became active and the circuit through the upper magnets was consequently broken at B_1, B_1 being pulled in towards the magnets of R_1. Even though the break at B_1 was only momentary, the chronoscope hands continued to run until a break at B_2 occurred, because once the arm of the chronoscope reached the lower magnets, the lower magnets exerted a much more powerful pull than the upper magnets on account of the distance factor.

Circuit Y was closed either at S-S or T-S. A make at S-S (stimulus-sound) resulted from the contact of the platinum strip with contact C_1 (or any other contact, if desired), the circuit being that from battery V_3 through M and relay R_3. A close at T-S (tapped-sound) was produced by a tap on the noiseless key K_4, the circuit being through battery V_4 and relay R_4. The closing of circuit Y produced a break at B_2 because Y then short-circuited relay R_2, thus allowing the arm B_2 to be pulled away from the magnets of R_2 by the relay's spring. The reclosing of B_2 did not cause the chronoscope hands to start again because B_1 had already closed and consequently the arm of the Hipp's chronoscope had already risen so as to be back again closer to the upper magnets, through which the current was stronger than through the lower ones. Thus B_2 could be opened and closed repeatedly without affecting the chronoscope hands. These could be made to start again only when the circuit through the upper magnets was again broken as a result of a break at S.

It will be observed that it was very easy, by a slight shift of connections, to connect first one contact, then another, with R_3 and thus by measuring in succession intervals S-C_1, S-C_2, etc., to determine by subtraction the intervals C_1-C_2, etc., *i.e.*, the intervals between any two contacts (or their resultant stimulus-sounds). It was in this way that the intervals between each pair of successive contacts was measured

in order to determine whether the contacts were spaced with exactness on the graduated rim of the Meumann's machine.

Data on the uniformity of the intervals between the sounds composing any stimulus-sequence are given below (Table I). The degree of uniformity attained will be described by giving information of two sorts: first, the average length of each of the intervals (the intervals between any two successive sounds); and second, the mean variation (MV) in each of these intervals. Data on the first point answer the question, how accurately were the different contacts spaced? For example, at the rate of one sound every 800 σ, five contacts were used, so that in one revolution there were five intervals. Each of these intervals was measured in the manner described, by means of the Hipp's chronoscope, the evening before this rate was used with subjects. If the

TABLE I

DATA ON CONSTANCY OF RATE OF THE STIMULUS-SOUNDS: THE AVERAGE LENGTH OF EACH OF THE INTERVALS PRODUCED BY ONE REVOLUTION OF THE ROTATING ARM OF THE MEUMANN'S MACHINE AND THE AVERAGE MV OF THESE INTERVALS

All values are given in sigma.

The values in the body of the table are the obtained average lengths of the intervals between the successive sounds of a series, each based on twenty measurements.

Rate	250.0	400.0	571.4	666.7	800.0	1000.0	2000.0	4000.0
Interval 1	249.9	400.8	571.1	665.0	798.4	999.3	2001.3	4000.0
" 2	251.7	399.8	571.8	666.5	801.5	1000.7	1998.7	
" 3	249.9	397.7	572.3	666.8	798.1	1001.9		
" 4	248.6	400.6	570.4	666.7	800.2	998.0		
" 5	252.1	401.8	570.0	668.1	801.7			
" 6	248.3	401.7	571.8	666.9				
" 7	252.2	399.2	573.0					
" 8	252.0	398.0						
" 9	248.0	400.9						
" 10	250.5	399.7						
" 11	247.5							
" 12	252.6							
" 13	247.9							
" 14	250.7							
" 15	247.4							
" 16	250.9							
Av. MV	1.4	1.3	1.9	1.7	1.3	1.4	1.4	1.7

first set of 10 measurements of each interval did not yield satisfactory uniformity, the contacts were very slightly readjusted in correspondence with the indications of the obtained measurements and a second, and in some cases a third, set of measurements were obtained. The averages given below are of course those obtained with the setting finally used. In the case of the mean variations, it is unnecessary to give data for each of the various intervals composing any one series, since the MV does not differ significantly for the different intervals of that series. It would not be expected to, since the MV of an interval is not appreciably affected by a change of only a few sigma in its average length. The only MV given, therefore, is the average of the MV's of all the intervals resulting from any one arrangement of the contacts. The average length of each interval, given in Table I, is the average of 10 measurements made at successive revolutions of the arm of the Meumann's machine. The data

given will be those obtained during the ascending series only, there being no appreciable difference between the uniformity attained in the ascending and descending series. Since at the rate of one sound in 4 sec, only one contact is used, the only information that can be given concerning uniformity at that rate consists of the mean variation. It should be stated that all the averages given below have been corrected to make the sum of the intervals in any one revolution equal to 4,000 σ. For example, if the sum of the intervals as measured was equal to 3,984 σ, it has been assumed that the chronoscope control was inaccurate to the extent of $-$ 16 σ in 4,000 σ or 0.4 percent and each average consequently has been multiplied by 1.004.

It will be noted that the obtained MV's do not increase much, if any, with increase in the length of the interval. Certainly the MV does not increase as the square root of the length. The conclusion is thus indicated that the variation arises mainly from inexactnesses at the beginning and ending of the interval, whether in the producing or recording (chronoscopic) apparatus, and not at all from variation in the rate of running of the hands of the chronoscope during the interval. These inaccuracies at the beginning and ending of the intervals depend upon details of the mechanical set-up, such as strength of currents, tensions of springs, etc., which are independent of the length of interval.

The synchronizing ability of each subject was determined by calculating his average error (C.E.) and the variability in this error (σ_{dis}). As already explained, by an error is meant the discrepancy in time between a stimulus-sound and the corresponding tapped-sound. By the average error is meant the algebraic average of the individual errors. In the present instance, this algebraic average may be termed the constant error (C.E.). When the sounds due to the subject's taps occurred before the corresponding stimulus-sounds, that is, when on the average they came too soon or ahead of time, the C.E. is labelled negative, or minus, and when they came too late, the C.E. is labelled positive, or plus. The standard deviation, used as a measure of the subject's variability, is the standard deviation of the distribution of the errors about their average. The errors were as a rule, in the case of every subject, distributed symmetrically about their own average.

The principal conclusions arrived at pertain to the C.E.'s and the σ's after they have each been expressed as percentages of the rate, rate being measured by the interval between any two successive sounds in the stimulus-series. Table II gives these relative or percentage values, both of the C.E.'s and the σ's. For example, at the rate of one sound every 4 sec, ascending series, subject By made a C.E. of $-$ 65.8 σ with a σ_{dis} of 216.60. In Table II these values appear as they become after

calculated as a percentage of the 4000 σ rate, namely, − 1.6 and 5.4. The conclusions hereafter stated refer to these percentage values.

The determination of the relation between variability, as measured by the σ, and rate constituted the central object of the experiment. When the results with all subjects are averaged it becomes immediately clear, as is evident from Table III, that there is a definite minimum in variability at the rate of one sound every 0.8 sec, and that the increase with the

TABLE II

CONSTANT ERRORS AND STANDARD DEVIATIONS FOR ALL SUBJECTS AT ALL RATES

Both C.E.'s and σ's are expressed as percentages of the rate.

Rate	Subject	Ascending		Descending		Average	
		C.E.	σ_{dis}	C.E.	σ_{dis}	C.E.	σ_{dis}
250.0	By	− 8.0	8.6	− 4.8	7.6	−6.4	8.1
	We	− 2.0	9.6	− 1.3	6.0	−1.7	7.8
	Lr	− 0.2	5.0	+ 2.2	5.5	+1.0	5.3
	Bl	−11.8	11.8	− 1.2	6.6	−6.5	9.2
	Mn	− 7.6	14.6	+ 8.2	5.0	+0.3	9.8
	Ke	+ 1.7	6.7	+ 3.9	6.2	+2.8	6.4
	Av.	− 4.6	9.4	+ 1.2	6.2	−1.8	7.8
400.0	By	− 1.3	5.7	− 3.8	5.1	−2.6	5.4
	We	+ 3.2	4.8	− 0.4	6.0	+1.5	5.4
	Lr	− 1.3	7.0	− 0.6	4.6	−1.0	5.8
	Bl	− 2.9	5.6	−12.3	7.5	−7.6	6.6
	Mn	− 1.5	6.8	+ 1.7	4.4	+0.1	5.6
	Ke	+ 2.2	5.5	− 0.4	4.5	+0.9	5.0
	Av.	− 0.3	5.9	− 2.6	5.4	−1.5	5.6
571.4	By	− 0.9	3.4	− 0.5	3.5	−0.7	3.4
	We	+ 1.3	4.2	+ 0.9	4.6	+1.1	4.4
	Lr	− 0.5	5.5	+ 0.2	3.4	−0.2	4.5
	Bl	− 3.0	4.6	− 3.5	4.6	−3.3	4.6
	Mn	− 2.2	5.1	+ 0.5	4.4	−0.9	4.7
	Ke	+ 1.7	5.0	− 0.1	2.9	+0.8	3.9
	Av.	− 0.6	4.6	− 0.4	3.9	−0.5	4.3
666.7	By	− 1.3	3.6	− 1.5	3.5	−1.4	3.5
	We	− 0.4	4.2	+ 0.1	3.9	−0.2	4.0
	Lr	− 1.2	4.2	− 1.4	3.4	−1.3	3.8
	Bl	− 4.3	4.5	− 3.5	4.2	−3.9	4.3
	Mn	− 6.6	5.0	+ 1.3	3.7	−2.7	4.3
	Ke	+ 0.6	3.4	+ 3.0	3.0	+1.8	3.2
	Av.	− 2.2	4.2	− 0.3	3.6	−1.3	3.9

TABLE II—*Continued*

Rate	Subject	Ascending		Descending		Average	
		C.E.	σ_{dis}	C.E.	σ_{dis}	C.E.	σ_{dis}
800.0	By	− 1.6	2.5	− 0.9	4.2	−1.3	3.4
	We	− 2.2	3.1	− 1.5	3.3	−1.9	3.2
	Lr	− 1.4	2.9	− 0.2	3.6	−0.8	3.3
	Bl	− 3.6	3.6	− 2.6	3.8	−3.1	3.7
	Mn	− 2.8	4.2	+ 1.0	3.5	−0.9	3.9
	Ke	+ 0.9	2.9	+ 1.0	3.1	+1.0	3.0
	Av.	− 1.8	3.2	− 0.5	3.6	−1.2	3.4
1000.0	By	− 1.3	3.6	− 1.8	3.3	−1.6	3.5
	We	− 1.2	4.7	− 1.2	3.4	−1.2	4.1
	Lr	− 0.4	3.2	− 0.4	3.9	−0.4	3.5
	Bl	− 2.5	3.6	− 1.7	4.3	−2.1	3.9
	Mn	− 1.8	4.4	− 0.9	4.2	−1.4	4.3
	Ke	− 0.1	4.0	+ 0.4	2.8	+0.2	3.4
	Av.	− 1.2	3.9	− 0.9	3.7	−1.1	3.8
2000.0	By	− 1.9	4.3	− 3.5	3.8	−2.7	4.1
	We	− 1.9	4.9	− 2.2	5.2	−2.1	5.0
	Lr	− 1.1	4.2	− 0.9	4.4	−1.0	4.3
	Bl	+ 0.6	5.0	− 1.5	3.9	−0.5	4.4
	Mn	− 1.7	4.4	− 2.6	4.8	−2.2	4.6
	Ke	− 0.7	4.2	− 1.6	4.8	−1.2	4.5
	Av.	− 1.1	4.5	− 2.1	4.5	−1.6	4.5
4000.0	By	− 1.6	5.4	− 1.6	6.6	−1.6	6.0
	We	− 3.2	6.3	− 2.2	6.8	−2.7	6.5
	Lr	− 3.5	6.1	− 2.7	5.3	−3.1	5.7
	Bl	+ 2.1	4.7	+ 2.1	4.6	+2.1	4.6
	Mn	− 0.3	7.3	− 2.4	7.5	−1.4	7.4
	Ke	− 0.5	6.2	− 0.2	5.8	−0.4	6.0
	Av.	− 1.2	6.0	− 1.2	6.1	−1.2	6.1

rates lying on each side of 0.8 sec progresses without exception in a regular manner. The standard deviation at the rate of 0.8 sec, at which it is a minimum, is 3.4 percent of the rate. This rises to 7.8 percent at the fastest rate and to 6.1 percent at the slowest rate. This increase in the relative standard deviation on each side of the most favorable rate is not, however, symmetrical, but is much more rapid as the rate becomes faster than as it becomes slower.

The difference between the average σ_{dis} at 800 σ and that at the rates on each side of this is small. The average σ_{dis} at the rate of 666.7 σ is 3.9 percent, at 800 σ, 3.4 percent and

at 1000 σ, 3.8 percent. The amount by which the first and last of these percentage σ's differ from that at 800 σ, the minimum, is only 0.5 and 0.4 percent, respectively. The question arises whether these differences are dependable, that is, statistically significant. The answer is that they are fairly significant. For example, the difference between the average percentage σ_{dis} for 800 σ and that for 1000 σ comes out 4.04 times the probable error of the difference;[2] for 800 σ and

TABLE III

SUMMARY, ALL SUBJECTS

Rate	Av. C.E.		Av. $\sigma_{dis\ (\omega)}$	
	Absolute	Percent	Absolute	Percent
250.0.....	− 4.4	−1.8	19.5	7.8
400.0.....	− 5.8	−1.5	22.5	5.6
571.4.....	− 2.8	−0.5	24.4	4.3
666.7.....	− 8.4	−1.3	25.9	3.9
800.0.....	− 9.3	−1.2	27.1	3.4
1000.0.....	−10.8	−1.1	39.0	3.8
2000.0.....	−31.6	−1.6	89.8	4.5
4000.0.....	−47.2	−1.2	242.0	6.1

666.7 σ, it is 4.97 times the probable error of the difference. The difference between all other rates and the 800 σ rate is of course much more significant. On the whole, then, it may be said that the data are adequate to indicate with fair dependability that on the average the percentage standard deviation at 800 σ is less than at any other rate which was used. This conclusion is further substantiated by the fact that each of the subjects except one, taken individually, showed a minimum relative variability at the 800 σ rate. The one exception, subject By, had a minimum which was exactly the same at two rates, namely 800 σ and 571.4 σ. For the rate between these two, the 666.7 σ rate, his relative variation was practically the same, *i.e.*, 3.5 percent, as compared with the minimum of 3.4 percent at the other two rates. One cannot,

[2] Calculated by first calculating the standard deviation of the average standard deviation, in the case of each rate, by the formula $\sigma_{\sigma\ dis} = \sigma_{dis}/\sqrt{2n}$, in which $\sqrt{2n} = \sqrt{2 \times 100} \times \sqrt{6}$, and then reducing to a percentage of the rate. The $\sigma_{diff.}$ is then calculated as the square root of the sum of the squares of the two values, and the P.E.$_{diff.}$ calculated by multiplying the $\sigma_{diff.}$ by .6745.

however, predict on the basis of the present data that all subjects will vary less at the 800 σ rate than at any of the other rates used in this investigation. It is only for the general trend of the amalgamated results of the six subjects here used that dependability may be claimed.

The foregoing discussion of the relationship between rate and variability is based upon the results of the ascending and descending series combined. Study of the ascending and descending series taken separately reveals that change in the rate of sequence of the sounds had a much greater effect upon variability in the ascending series than in the descending. The descending series by itself shows no reliable effect of change in rate between the limits 571.4 σ and 1000 σ, inclusive. Since the ascending series came first, the descending series consists of results obtained at a more advanced stage of practice than the ascending. The lesser effect of rate in the descending series thus suggests that with practice there may occur on the part of the subject an adaptation or adjustment to rate, within certain limits, so that the subject is able to do as well at one rate as another.

In order to test this hypothesis that with practice the subject may adapt to a certain medium range of rates, further trials were given all subjects with the three rates, 1000 σ, 800 σ and 666.7 σ. These trials were conducted in exactly the same manner as already described and constituted simply a complete repetition of the work with these three rates, except that in this further work the descending series, beginning with 1000 σ, came first and the ascending series, beginning with 666.7 σ, came second. This further work, to determine the effect of practice, was initiated (with the 1000 σ rate) three days after the last rate used in the descending series of the main experiment (250 σ). The results obtained are given in Table IV.

The results given in Table IV confirm those of the descending series of Table II in that they indicate that change in rate within the limits 666.7 σ and 1000 σ produces no significant effect. Thus the conclusion that the variability of the subjects has a minimum at the 800 σ rate is borne out only

TABLE IV

RESULTS OBTAINED WITH REPETITION OF THE EXPERIMENT (PRACTICE) WITH THREE RATES

Both C.E.'s and σ's are expressed as percentages of the rate.

Rate	Subject	Descending		Ascending		Average	
		C.E.	σ	C.E.	σ	C.E.	σ
666.7	By	−1.6	3.2	−2.1	3.6	−1.9	3.4
	We	−0.3	3.5	−0.1	4.3	−0.2	3.9
	Lr	+1.3	3.5	−1.4	3.3	−0.1	3.4
	Bl	−2.3	3.7	−3.2	3.6	−2.8	3.6
	Mn	+0.9	5.1	+0.9	4.2	+0.9	4.6
	Ke	+2.5	3.3	+2.2	3.1	+2.4	3.3
	Av.	+0.1	3.7	−0.6	3.7	−0.3	3.7
800.0	By	−4.2	4.8	−2.7	4.7	−3.5	4.7
	We	−0.4	3.8	−0.3	3.8	−0.4	3.8
	Lr	+2.8	3.5	+0.4	3.7	+1.6	3.6
	Bl	−3.2	2.6	−2.4	2.9	−2.8	2.8
	Mn	−1.0	4.9	+1.2	3.8	+0.1	4.4
	Ke	+4.3	3.8	0.0	3.0	+2.2	3.4
	Av.	−0.3	3.9	−0.6	3.6	−0.5	3.8
1000.0	By	−1.8	4.0	−1.1	3.2	−1.5	3.6
	We	+0.1	3.9	−0.7	4.0	−0.3	3.9
	Lr	−0.1	3.3	−0.5	3.6	−0.3	3.4
	Bl	−2.1	4.1	−2.2	3.9	−2.2	4.0
	Mn	+3.2	5.0	+1.8	4.6	+2.5	4.8
	Ke	+1.0	2.8	+0.8	2.8	+0.9	2.8
	Av.	0.0	3.8	−0.3	3.7	−0.2	3.8

by the first series, an ascending series. It is true that the following descending series also shows a minimum, but the minimum instead of being definitely localized at one rate spreads over a stretch including at least the two rates of 666.7 σ and 800 σ and with less strictness all the rates from 571.4 σ to 1000 σ inclusive. The data do not establish the cause of this broadening of the range of rates at which the minimum variability occurs. They do indicate, however, that the change is due to the mere continuation of the sittings, with the practice which results therefrom, rather than to any effect of ascending as compared with descending series; for the last series, which was an ascending one, agrees almost perfectly with the second series which was a descending one. The most natural explanation appears to be one which assumes

that as the subject acquires experience with the various rates he becomes so accustomed or adapted to them that they are equally favorable. Such an explanation would be in harmony with the well known effect of frequent repetition of an interval upon the tendency of the subject to judge it (absolutely) short, long or "adequate." Apparently, however, this becoming adapted to a certain range of rates decreases somewhat the degree of initial adaptation to the most favorable rate. In other words, the subject in learning to synchronize equally well to several rates apparently adopts a set or attitude which in the case of rates at first not quite maximally favorable is a better adaptation but which to the originally most favorable rate is a less perfect adaptation. Otherwise it is hard to explain why with the 800 σ rate the results of the first series were never equalled in the later series. That this result could be due simply to a growing carelessness or weakened motivation on the part of the subjects seems hardly possible since the rates of 666.7 σ and 1000 σ do not reveal the slightest tendency towards decreased accuracy.

As regards the C.E.'s, there are three chief conclusions established by the data of Tables II and III. First, the C.E.'s show little, if any, dependence upon rate. It is true that the relative C.E. becomes suddenly smaller at the 571.4 σ rate, but the drop here is so sudden and out of line with the general tendency that it must be regarded as almost certainly fortuitous.

Second, the C.E.'s are preponderantly negative. Of the 96 sittings comprising the main experiment, 73 yielded negative C.E.'s and 23 positive ones. Of the 16 C.E.'s of each subject, the great majority were negative in the case of all subjects but one. In the case of this one subject there were 7 negative C.E.'s and 9 positive ones. On the whole, then, the subjects tapped so that their tapped-sounds anteceded the stimulus-sounds. This result is in agreement with the previous investigation by Dunlap,[3] an investigation which was extensive, including both visual and auditory stimulus-series, but which, as the author himself emphasized, was not carried

[3] K. Dunlap, *op. cit.*, p. 416.

out systematically enough to establish definite conclusions beyond what was referred to as the similarity between the synchronization experiment and the complication experiment.

Third, the variation in a subject's C.E.'s from sitting to sitting is rather pronounced, and since there is very little if any relation between the C.E.'s and the rate, this variation must be due to variations which, in spite of careful instructions, occurred from day to day in the subject's sets or attitudes. This matter appeared so important that a special statistical procedure yielding what the writer terms an "index of quotidian variation" was employed for its study. By the index of quotidian variation is meant the ratio of the variability actually shown from sitting to sitting to the variation which would be expected, according to the theory of sampling, on the basis of the variability of any one day. It will here suffice to say that in the case of at least 4 of the 6 subjects (namely, subjects We, Bl, Mn and Ke) the constant errors at different sittings differed from each other more than could possibly be accounted for by the theory of sampling. In other words, the subject's responses at different sittings must be regarded as trials drawn from different categories or orders, or, in more psychological language, as responses executed under the influence of different attitudes or ways of functioning.

Just what shifts in attitude caused these significant variations in a subject's constant errors is not known. It might be expected that the introspective reports would furnish an explanation. This does not appear to be the case, though it is true that a study of these reports brings out a number of features in which the reports from day to day show variation. Just how relevant these variations are, however, that is, just what effect they had upon the accuracy of synchronization, cannot be determined. Among the reported attitudinal variations of a given subject from one sitting to another are the following: variation in mode of apprehending the sounds, *e.g.*, as a unified or rhythmical series or as a series of separate taps; variation in strain sensations from hand, wrist, shoulder, larynx, eyes and brow and in breathing, or variation in the degree of attention to the same; counting or not counting; variation

in confidence, fatigue, and general alertness; difference in the degree to which tapping was what the subject called automatic and what he called voluntary; variation in the tapping movements; variation in the distribution of attention between the tapping movements or sensations therefrom and the sounds; and variation in the effort, and the promptness with which this effort was made, to get back into time again upon observing a lack of synchronization, *i.e.*, variation in the tendency towards perseveration in an adopted rate even after it has led to observable lack of synchronization; etc.

Apart from any bearing upon the matter of variability, there is one point brought out by the introspections which seems particularly important. For all subjects the task of synchronizing at the rates of one sound every 2 sec or every 4 sec seemed definitely different from that at rates of one sound every 1 sec or less. Probably all subjects did not experience the difference between the slower and the faster rates in exactly the same way, but there appears to be a similarity in the differences reported of a sort to suggest dependence upon some one fundamental difference. This difference between the faster and slower rates appears in the fact that in the case of the faster rates the series are described as more unitary or more rhythmical, and the task of synchronization is reported to be more automatic, easier and more certain, than in the case of the slower rates. The following extracts taken verbatim from the subjects' comments clearly indicate this difference:

Ke, 800 σ, descending series: "Yesterday [1000 σ] and today I have felt that the tapping became more automatic on my part. It is as if I could 'comprehend' this rate, and once started keep on regularly and automatically, without paying such strict attention to the stimulus-sounds. In contrast with this, with the longer intervals I felt as if each tap were an individual thing—not one of a series, and each response I made was separately willed. Today it seemed as it did when we began the experiment. Once my hand and arm started, it adopts a certain rate and the tapping becomes mechanical. I feel certain I am much more accurate, too."

Ke, 4000 σ, descending: "I never seemed to act automatically as I did with the shorter intervals. Each tap was an individual reaction. . . . I couldn't get the 'feeling' of the rate or perform with any feeling of satisfaction the task of synchronizing."

Ke, 570 σ, ascending: "The rate today seemed easier to duplicate than those faster ones of the first two days. I felt as though my arm became an automatic machine after I had tapped for a while, when I seemed to be doing best. Then I did not

voluntarily make each tap but it went on without my will—all I had to do was to set my arm in motion at the correct rate and it went ahead tapping. Twice I had an impulse to count—like 1-2-3-4, 1-2-3-4, etc., but quickly inhibited it and it did not bother me."

Ke, 2000 σ, ascending: "It seemed *very* hard today to synchronize the taps. I wanted to count *one two* between each of my taps and the effort to inhibit this tendency kept distracting me. If it wasn't *one two*, I wanted to say *I am* or *you bet* or some other combination of two words. The times I made the taps coincide seemed relatively few. The rest of the time I was either ahead of the stimulus, or heard it and then reacted."

We, 800 σ, ascending: "The tendency was to perform the tapping automatically."

We, 2000 σ, ascending: "It was an unpleasant process—the fixed stimulus-series seemed variable, with nothing but unoccupied time between any two succeeding sounds. It almost seemed a matter of chance when I tapped."

We, 4000 σ, ascending: "It was just a matter of 'blind' guessing when to tap the key."

Mn, 570 σ, ascending: "While tapping I would lean forward listening to the sound and the only consciousness I had of my finger movements was that I was tapping harder than I needed to."

Mn, 4000 σ, ascending: "The slow rate with long intervals is very difficult. The taps were so far apart that I did not get the feeling of a series with equal intervals between taps. I could not feel the unity of the series and this is what seemed to make it so difficult."

Bl, 800 σ, ascending: "The action of the tapping finger seemed to be controlled by the stimulus-series and more or less free from conscious control. In other words it became automatic, if that expression may be used."

Bl, 2000 σ, ascending: "There was a feeling of uncertainty. It was difficult to synchronize. It was almost impossible to keep from straining towards a rhythm which one could not, however, quite succeed in setting up."

By, 470 σ, ascending: "An auditory pattern is formed by the sounds of the stimulus-series. No attention is paid to the motor manipulation of the key. Both sets of clicks form into one pattern. Sometimes the pattern becomes so unitary that I stopped to see whether the guide sound [stimulus-sound] was continuing."

By, 2000 σ, ascending: "The sound stimuli came at such long intervals that it was impossible to fixate a pattern at first. . . . A pattern was becoming right well stamped in at the end of the sitting."

By, 4000 σ, ascending: "They all [the intervals] were too long—I could not seem to wait so long."

Lr, 400 σ, ascending: "With me, it is almost impossible to avoid rhythm."

Lr, 800 σ, ascending: "Difficulty in avoiding rhythm does not seem to be becoming less."

Lr, 4000 σ, ascending: "It seems almost impossible to keep the taps synchronous. There was no basis apparently for noting the time interval. There was no rhythm so no attempt was necessary to avoid one."

There is a striking similarity between some of the above comments and reports obtained from a different group of subjects in experiments by the writer on the reproduction of empty temporal intervals,[4] in which the introspective reports

[4] The Reproduction of Temporal Intervals. This JOURNAL, 13, 1930, p. 494.

of the subjects brought out the fact that "the experience of duration becomes definite only with the longer time-intervals, that is, those beyond 1.5 sec." This indication of the introspective reports appeared to harmonize with the fact determined from measurements that a marked increase in the percentage variability of the reproductions of empty intervals occurs in the region between 1.5 and 4–6 sec. Both sets of results may perhaps be regarded as in line with the fact that the rhythmicalness of a series of sounds begins a rapid decline at the rate of about one sound every 2 sec, or in the case of two-beat measures, a rhythmical measure of 4 sec.[5]

Another similarity between the data on synchronous tapping and those on the reproduction of temporal intervals lies in the location of the minimum standard deviation. In the present experiment on synchronous tapping this minimum, taking the average for all subjects, was found to lie at 0.8 sec, while in the experiment on the reproduction of empty intervals the minimum was 0.6 sec, with the second smallest variation at 0.8. The two results are not in exact agreement but certainly very close to it, especially in view of the fact of considerable individual variation, particularly in the case of the reproduction experiments, and the further fact that in neither experiment is there more than a very small and scarcely reliable difference between the standard deviation at the most favorable rate or interval and that of the adjacent rates or intervals.

An interesting question is, what is the slowest rate, or the upper limit, at which synchronization is possible? In this form the question is unanswerable because the definition of the upper limit of synchronization is left too vague. Synchronization is by no means perfect at any rate; and on the other hand, no matter how slow the rate, one can always react to each stimulus-sound or he can guess when each sound is due and tap accordingly. The question of the upper limit of synchronization resolves itself into the answerable question, when does the attempt to synchronize cease to enable the subject to produce smaller errors of synchronization than he

[5] H. Woodrow, A Quantitative Study of Rhythm. *Archives of Psychol.*, Whole No. 14, 1909, pp. 35 and 36.

would make if he simply reacted to each sound as in the reaction-time experiment? In phrasing this question it is understood that by error of synchronization is meant not the C.E., which is the algebraic average error, but the average crude error, *i.e.*, the average of the errors neglecting their sign. Thus if a subject tapped half the time 100 σ too late and half the time 200 σ too soon, his C.E. would be $-$ 50 σ, but his crude average error would be 150 σ. In order to determine whether this error is greater than that which the subject would make if he made no attempt to synchronize but tried merely to react, one should use data from that form of the reaction-time experiment in which the subject reacts to each sound in a continuous series, the sounds of which are evenly spaced and in which each sound serves as the warning or ready signal for the next sound. Now such reaction-time experiments have not been made with the same subjects as used in the present synchronization experiment, since the different attitudes required by the two experiments would very likely interfere with each other. However, experiments made by the writer on other subjects, and previously published,[6] indicate that in the case of reactions to each of a series of regularly repeated sounds it is exceedingly unlikely that the reaction-time of a normal subject would be over 180 σ or under 120 σ for a series of sounds of the loudness of the stimulus-clicks used in the present synchronization experiment, if the sounds occurred at the rate of one sound every 3 sec. In other words, if a subject did not attempt to synchronize at all, but simply reacted to each sound as it came, he would beyond reasonable doubt make an average error of between 120 σ and 180 σ. Now no subject made an average error of synchronization at the rate of one sound every 2 sec which even remotely approaches these reaction times, whereas at the rate of one sound every 4 sec every subject made an average error in excess of them. The average crude error of synchronization at the 2 sec rate was 77.4 σ, whereas at the 4 sec rate it was 208.3 σ. The average crude error made by each subject for the 2 and 4 sec rates are shown in Table V.

[6] H. Woodrow, The Measurement of Attention. *Psychol. Monog.*, 17, Whole No. 5, 1914, p. 33.

TABLE V

AVERAGE CRUDE ERRORS OF SYNCHRONIZATION IN SIGMA AT THE 2 SEC AND 4 SEC RATES

	2 Sec Rate			4 Sec Rate		
	Ascending	Descending	Av.	Ascending	Descending	Av.
By....	72.3	78.9	75.6	175.0	212.4	193.7
We...	80.8	84.9	84.4	213.6	217.5	215.6
Lr....	68.6	72.7	70.7	215.5	193.2	204.4
Bl....	80.2	66.4	73.3	196.5	177.7	187.1
Mn...	77.3	86.6	82.0	250.3	249.0	249.7
Ke....	72.8	84.7	78.8	213.5	184.8	199.2
Av....	75.3	79.5	77.4	210.7	205.9	208.3

Assuming that the reaction-time to a regularly repeated sound at rates around that of one sound every 3 sec lies somewhere in the range of 120–180 σ, and further defining the upper limit of synchronization as that rate at which the average crude error is as large as the afore-mentioned type of reaction-time, it is clear that for every one of the 6 subjects the upper limit of synchronization lies between the 2 sec and 4 sec rates. This is a rather roughly determined limit, and the data at hand do not admit of reliable interpolation, because the reaction-time of the identical subjects used in the synchronization experiment is not known. It seems a safe conclusion, however, that the upper limit of synchronization, as defined, lies at least as close to the 4 sec rate as to the 2 sec rate. If an average reaction time of 165 σ is assumed, the upper limit of synchronization yielded by simple linear interpolation would lie at the rate of $2 + [165–77.4/208.3–77.4 \times 2]$ sec or at the rate of one sound every 3.34 sec. At this rate the average error of synchronization and the average reaction-time would be identical.

It is an interesting speculation whether this value has any relation to the maximum duration of a rhythmical group. Now it is possible to determine the upper limit in the rate at which intensity or duration exert any rhythmical effect in the sense of promoting temporal segregation. By applying the method of minimal changes to the determination of this limit, the writer arrived at the conclusion that the segregating effect

of objective accentuation of alternate sounds practically ceased in the case of two-beat measures when the duration of the measure reached 7.0 sec.[7] Since two-beat measures of this length are the equivalent of one beat every 3.5 sec, it is obvious that we have here to deal with intervals that are at least of the same order of length as those between sounds at the rate representing the upper limit of synchronization, namely, a rate over 2 sec and under 4 sec and roughly estimated to be for the average subject in the neighborhood of 3.34 sec.

A relation between the upper limit of synchronization and the upper limit of duration of a single rhythmical foot would be in line with other relations that have been suggested in the preceding pages between synchronization and the perception of rhythm and time. It has been mentioned, for example, that the rates which give the smallest variability in synchronization errors, namely, 666.7 σ–1000 σ, are exceedingly close to those at which the errors of reproduction of empty time intervals were found to show the smallest variation, namely, 600 σ and 800 σ. It has further been suggested that the marked change in the type of introspective report which occurs as the rate increases from 1000 σ to 2000 σ or beyond is connected with the fact that introspective reports on empty time intervals also show a striking change, beginning at about 1500 σ, a change which is accompanied objectively by an increase in the variability of reproductions. It is further suggested that this region in which both accuracy of synchronization and accuracy of reproduction of empty intervals begin a rapid deterioration is apparently very close to the duration of a single rhythmical foot beyond which the rhythmicalness of a sound-series begins rapidly to decline. There thus appears to be one duration, at about 1.5–2 sec, at which the reproduction of empty intervals, synchronization and the experiencing of rhythm all begin to become difficult and another duration around 3.4 sec (2–4 sec) which represents the vanishing point of the capacity for synchronization, and (if taken as the duration of a single foot) for experiencing rhythm. The ability to reproduce empty time intervals, it is true, does

[7] A Quantitative Study of Rhythm. *Archives of Psychology*, 14, 1909, p. 64.

not disappear at 3.4 sec. It is, however, barely possible that there may be some connection between a duration of twice this length, namely 6.8 sec, which is near the upper limit of a rhythmical measure, and the fact that in this neighborhood the increase in relative variability in the reproduction of empty time intervals, which begins at 1.5–2 sec, appears to reach its maximum, to remain approximately constant up to intervals of at least 30 sec.[8] It may possibly be identified with what some writers call the maximum duration of the perceptual or specious present, said to be about 6 sec.[9]

Summary

1. Of the rates used in this study, the rate of one sound every 800 σ resulted in the smallest variation in the errors of synchronization. When the variability of a subject was measured in terms of the standard deviation of his errors, and this standard deviation expressed as a percentage of the rate, that is, as a percentage of the interval between two successive sounds in the series, it was found that on the average the standard deviation at the rate of one sound every 0.8 sec was 3.4 percent; and that on the average the standard deviation increased regularly on both sides of this most favorable rate until at the rate of one sound every 4 sec (the slowest rate used) it had reached 6.1 percent and at the rate of one sound every ¼ sec (the fastest rate used) 7.8 percent.

2. It was found that as practice was continued the subjects became adapted to a range of rates, extending from at least 666.7 σ to 1000 σ, so that on the average they manifested the same variability for all the rates within this range. This sameness in variability came about through a slight increase in variability with the originally most favorable rate (800 σ) together with a slight decrease in variability at the adjacent rates (666.7 σ and 1000 σ), suggesting that adaptation to an increased range of rates was attained only at the cost of a slight lessening in the degree of adaptation to the originally most favorable rate.

[8] H. Woodrow, The Reproduction of Temporal Intervals. This Journal, 13, 1930, pp. 492 and 499.

[9] *Cf.* Elements of Human Psychology, by H. C. Warren and L. Carmichael, 1930, p. 163.

3. At some rate between that of one sound every 2 sec and one sound every 4 sec, the errors of synchronization (average crude errors) became as large as the estimated reaction-times in the case of reactions to each sound in a series given at the same rate. This rate is termed the upper limit of synchronization. It is roughly estimated that on the average it lies in the neighborhood of the rate of one sound every 3.34 sec.

4. The constant errors showed little, if any, dependence upon rate, either as regards their magnitude or their direction.

5. The constant errors were preponderantly negative, that is, there prevailed a strong tendency for the sounds produced by the subjects' taps to occur too soon, *i.e.*, before or ahead of the sounds of the stimulus-series with which they were intended to synchronize.

6. Variation in a subject's performance, after eliminating the effect of rate, was rather pronounced from sitting to sitting.

7. The subjects' introspective reports indicate that the task of synchronizing is a different kind of a task at the two slowest rates used than at the faster rates.

In addition to the above conclusions it may be mentioned that a new chronoscopic set-up was employed in this study and found to afford a satisfactory technique for measuring the accuracy of synchronization.

(Manuscript received April 14, 1931)

A STUDY IN PHOTOPIC ADAPTATION

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At least three generalizations are prevalent in the literature and textbooks regarding the course and end-results of photopic adaptation which seem to have been accepted by most writers on the subject. They are as follows: (1) color stimuli after acting upon the retina for a long time gradually fail to produce color perceptions; (2) a 'neutral' or mid-gray represents the visual quality which all colors and brightnesses assume after 'complete' adaptation; (3) upon removal of the stimulus to which the retina has become adapted, an after-image having the complementary color appears. It is even asserted, without qualification, that if colored glasses are worn external objects are sooner or later seen in their natural colors, the supposition being that after adapting to the color of the glasses its presence is no longer sensed and hence all colors appear in their natural modes.[1] While these generalizations are based upon certain facts which we do not deny, it must be stated at the outset that without the backing of the Hering theory of vision, which is the last word for many psychologists on the theory of vision, these so-called laws of adaptation would not have gone unquestioned so long and so generally. A critical examination of the laws themselves and consideration of certain facts in the literature make the laws as stated without rigid qualifications either meaningless or untrue.

[1] One quotation will suffice to convey the views usually held by psychologists: "Colour stimuli, when acting for a long period on the retina, gradually fail to produce colour sensations. When, for example, coloured glasses are continually worn, external objects are sooner or later seen in their natural colours. Upon removal of a colour stimulus to which the retina has become adapted, an after-image of the complementary is developed," C. S. Myers, *A Text-Book of Experimental Psychology*, 1928, 74.

The conditions upon which the laws of adaptation, as given above, depend may be summarized as follows: (1) stationary fixation; (2) constant light flux upon the retina; (3) stimuli of medium intensity; (4) local stimulation rather than general; (5) if glasses are worn, glasses which are not strongly selective, as will be shown in this study. These conditions are seldom stated as being necessary for the validity of the laws of adaptation and judging by the form the discussions take one might be led to believe that complete adaptation takes place with stimuli of any intensity and saturation, with any sort of colored glasses if only worn sufficiently long,[2] and that the end-product of adaptation is a neutral gray in accordance with the Hering theory of vision. Various writers have maintained each of these propositions.

Burch [3] and Sheppard,[4] on the basis of their experiments, have maintained that all colors go to neutral gray no matter what the hue, saturation or brightness of the stimuli. Sheppard believed eye-movements to be responsible for his failure to achieve complete adaptation and Ferree [5] agreed that eye-movements might allow recovery from adaptation by freeing the retina from stimulation and facilitating the "streaming phenomena" which replenish the retinal substances responsible for colors. Troland has been almost alone in asserting that with saturated colors adaptation does not result in com-

[2] That these effects of adaptation are held to be universal in the manner stated cannot be doubted in the light of the discussions of the subject in the literature, of which the quotation from Myers, given in note 1, is a fair sample. E. B. Titchener has expressed the Hering view as to the final result of adaptation in the following words: "If we are exposed, for some length of time, to visual stimuli of approximate constancy, our eyes become 'adapted' to their surroundings; we 'grow used' to the particular visual environment. The law of this adaptation is that all brightnesses tend toward middle gray, and all colours towards neutrality," *Experimental Psychology*, 1, i, 1901, 22.

[3] G. J. Burch, *Phil. Trans.*, 1899, 191B, 1–35.

[4] H. Sheppard, Foveal adaptation to color, *Am. J. Psychol.*, 1920, 31, 34–58. Hering had the same view also. See his *Grundzüge der Lehre vom Lichtsinn*, 1920, 100 ff., especially 115. For Hering's account of the effect of eye-movements upon adaptation, *ibid.*, 272.

[5] C. E. Ferree, An experimental examination of the phenomena usually attributed to the fluctuation of attention, *Am. J. Psychol.*, 1906, 17, 82–120; *ibid.*, The intermittance of minimal sensations, 1908, 19, 58–129. For the streaming phenomena see the same author, *ibid.*, 1908, 19, 484–503.

plete extinction of chroma and the perception of a visual mid-gray. He has found that white may achieve a final end-state which is far too light a quality to be classed as a 'mid-gray.' [6] One writer has reported that light from 490–460 mμ appears to shift toward that of the shorter wave-lengths, and light of 560 mμ showed no change in hue.[7] Evidence thus exists contrary to the usual views held regarding the course of adaptation and this evidence constitutes a direct challenge to the theory of Hering.

But so far no one, to our knowledge, has pointed out the absurdities which lurk in the statement that if colored glasses be worn for a sufficiently long time, all objects are seen in their natural colors. These absurdities arise from the fact that *strongly* selective glasses may be used: for example, glasses which transmit light of so restricted a wave-length region that this light is equivalent to an homogeneous light-stimulus. Light which passes through such glasses can vary significantly in only one respect—intensity. Two surfaces whose reflectances for such light are identical will appear to be identical, and no amount of adaptation can possibly make them appear other than identical. But the spectral reflectances of two such surfaces for wave-length regions different from that transmitted by the glasses are entirely unrestricted; there is virtually no restriction on the hue of the color which may be evoked by such a sample. In other words, two samples of whatever hues desired may be chosen so that they will appear identical viewed through the glasses, and this is plainly not equivalent to seeing objects in their natural colors.

With *mildly selective* glasses adaptation by reducing the dominance of a given component may restore objects more or less to their natural appearance, but, as we shall show, adaptation does not proceed beyond a certain point in reducing colors and does not fully replace colors whose stimuli strongly selective glasses practically exclude from the eye. There can therefore be no question of 'natural' appearances with strongly

[6] L. T. Troland, The colors produced by equilibrium photopic adaptation, *J. Exp. Psychol.*, 1921, 4, 344–390.

[7] H. Voeste, Messende Versuche über die Qualitätsänderungen der Spektralfarben in Folge von Ermüdung der Netzhaut, *Z. f. Psychol.*, 1898, 18, 257–267.

selective glasses when all the facts are taken into consideration. Even Troland, with whom we are in otherwise complete agreement, is inclined to accept the results reported in the literature with colored glasses as contrary to his findings, seeming at a loss to reconcile them with his results because he fails to note that if complete adaptation occurred, it must have been with mildly selective glasses which transmit the entire spectrum, and hence furnish no evidence against his conclusions.[8]

Finally we must note that generalizations concerning adaptation include some statement to the effect that adaptation to one color increases sensitivity to the complementary color. This opposite effect of adaptation can be easily demonstrated and furnishes one of the main props of the Hering theory, for according to it opposed metabolic processes predominate after the metabolic processes associated with a given color have run their course through adaptation. But at least two effects have been reported in the literature which do not harmonize with either the so-called law of 'opposite sightedness' following adaptation or the Hering theory: (1) Burch found that after adaptation to a given color the color persisted often as a luminous fog over the whole field of vision after it had 'disappeared in sensation'; (2) Troland failed to get a negative after-image if the eye was kept in darkness after exposure to the stimulus for adaptation, a fact which we have verified in this study.[9] The continued presence of the originally given color and the absence of the negative after-image under certain conditions are both contrary to the theory of opposed processes as the basis for color vision under all conditions.

[8] *Op. cit.*, footnote 6. He refers to the results of Maria Bokowa who reported complete adaptation with colored glasses. In spite of the fact that she stated that glasses should be used which transmit only a single wave-length, or rather a very narrow region from the spectrum, the glasses she actually used were made with cuprous oxide. We venture to say that judging from her results her glasses were not highly selective. Other red glasses whose coloring material is copper transmit some light of all wave-lengths. See, for example, an article by K. S. Gibson, E. P. T. Tyndall, and H. J. McNicholas, The ultra-violet and visible transmission of various colored glasses, *B. S. Tech. Paper*, 1920, No. 148, 10. Bokowa's work will be found in *Z. f. rationelle Med.*, 1863, 17, 161–165.

[9] Burch and Troland, *opp. citt.*

It is evident that the classic laws of adaptation hold, as Troland has pointed out, for the case of 'complete adaptation' only under special and limited conditions and, in the light of all the facts in the literature, should not be stated as generalizations without adequate qualifications. Two important points, however, still call for special study even if the laws are restricted to the conditions mentioned above: (1) does complete adaptation with mid-gray as an end-result occur if the retina is subjected to constant stimulation and eye-movements are allowed? (2) does complete adaptation take place with all colored glasses?

The first point is raised because it has generally been assumed that stationary fixation furthers adaptation by providing constant stimulation. Eye-movements are thought to hinder adaptation by freeing the retina from stimulation. This is possible if stimuli are used which affect only a small portion of the retina. In the case of general stimulation of the whole retina the view has been held that eye-movements result in unequal stimulation of the retinal elements, which is indeed true if special means are not taken to insure equal stimulation for every part of the retina. The first part of this study was designed to produce conditions in which adaptation could be studied when the whole retina is subjected to constant stimulation even though the eye is allowed to move. By this means we hoped to settle a number of questions concerning the course of adaptation and the rôle played by general stimulation, eye-movements, and more normal conditions of observation, since stationary fixation involves more or less strain and cannot be held for more than a very few minutes at most. The second part of this study was made with the use of colored glasses whose transmissive properties are known and are such that we are in a position to explain when complete adaptation occurs with colored glasses and when not, thus reconciling statements made in the psychological literature with other known facts of color vision and the facts of physics.

EXPERIMENTAL

Part I

Procedure.[10] In order to keep the amount of light falling upon all parts of the retina constant while allowing eye-movements, the following apparatus and procedure were employed. A hollow sphere, 36 inches in diameter, lined with orange-red Hering paper was constructed. Through an aperture in the top wires for the light stimulus were admitted so that a 100-watt Mazda lamp could be hung with its filament quite near the center of the sphere. A hole in the bottom of the sphere permitted the *O* to insert his head. It is evident that this apparatus was designed so that an *O* could be supplied with a reasonably uniform visual field, binocularly viewed. Since the *O*'s eyes were placed approximately at the center of the sphere all parts of the surface were nearly at the same distance from his eyes. On this account eye-movements could be permitted without fear of important changes in pupil size. The field was reasonably uniform because: (1) the source being viewed possessed nearly identical spectral reflectances; and (2) all parts of the surface receiving light were practically equidistant from the source. Hence, regardless of eye-movements the flux on all parts of the retina may be regarded as approximately constant. Had ideal conditions been completely realized, we would have had: (1) a perfect point source instead of an ordinary concentrated filament lamp; (2) a true sphere instead of an approximate one built up (orange-peel fashion) of plane, cardboard strips about two inches wide at the center; (3) a perfectly uniform inner surface instead of a surface revealing details of construction (edges of the colored strips, paste, stains, *etc.*); and (4) an *O* either negligibly small or perfectly transparent, instead of an *O* whose head interfered somewhat with a uniform distribution of light by multiple reflection within the sphere.

The ease with which the surface, 18 inches from the *O*'s eyes, could be fixated with both eyes if fixation was desired and the lack of need for fixating any particular point resulted in practically constant retinal illumination without the use of artificial pupils or any other special devices for the same. The course of adaptation could thus be studied under conditions which might be described as normal rather than the usual artificial conditions involving monocular vision, attempts at stationary fixation, strain incident to long fixation and an unnatural mode of functioning in general. The apparatus in spite of its imperfections fulfilled these demands with a high degree of success. It enabled us to observe under more nearly constant conditions, both as regards the stimulus and the flux of light upon the retina, for a longer period of time than has heretofore been possible. At the same time the rôle of eye-movements in adaptation could be studied while all other factors were kept constant.

The observations were made by the writers in a number of periods ranging from 5 to 75 minutes of continuous adaptation or exposure. The light reaching the eye with the 100-watt Mazda lamp as source was very intense. This had the double effect of (1) reducing fluctuations in pupil size,[11] and (2) hastening adaptation which proceeded at a much faster rate than with weaker stimuli. By the end of periods as long as an hour considerable eye-strain and even headache were experienced, in spite of free eye-movements.

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[10] The experiments in this part of the study were performed at Cornell University during the year 1924–25.

[11] With intense stimulation contraction of the pupil is at a maximum. There is really no such thing as an absolutely constant natural pupil as anyone may verify by observing it through a small magnifying glass.

Results. We may summarize our findings with the 'adaptation sphere' as follows:

(1) At no time, not even after 75 minutes' adaptation, did the color of the surface viewed completely and permanently disappear. The equilibrium condition with its associated neutral gray demanded by the Hering theory under conditions of constant retinal stimulation failed to appear. This result substantiates the work of Troland who failed to get complete adaptation with stationary fixation and saturated colors.

(2) The red component tended to be reduced making the orange surface more yellowish—a finding in harmony with Voeste's work which showed that light of wave-length from 660–560 mμ appeared to be shifted toward the shorter wavelengths in adaptation.[12]

(3) At brief intervals lasting not more than a few seconds if the eyes were held fixed a fairly good gray was observed which appeared more or less rhythmically and alternately with orange and yellow. It would appear as if the Hering neutral state may be reached with stationary fixation for extremely short periods. While the rhythm seemed to correspond to the rhythm of the pulse it may have been associated rather with pupillary changes. But our findings seem to show that these rhythms were not due to pupillary changes for

(4) The negative after-image never appeared, showing that the pupil was of nearly constant diameter throughout the periods of observation since, as Troland has pointed out, pupillary changes may be responsible for after-images as well as an actual 'dimming' of the stimulus-field.

(5) Whatever adaptation took place occurred in the first few minutes of observation. Beyond the first few minutes continued observation failed to reveal signs of progressive adaptation beyond the points indicated in (2) and (3) above.

Part 2

Procedure. Colored glasses were worn by a number of *O*s who reported upon general features of the landscape, special objects and a selected set of colored papers

[12] Troland believes that Voeste's experiments involved the Bezold-Brücke effect and that there is no change in hue (with unitary colors only?) with adaptation. See his *Psychophysiology*, vol 2, Sensation, 1930, 180.

during the period of observing. For colored glasses a pair of diving goggles equipped with rubber hoods fitting snugly against the eyes were used. Over the clear glasses of the goggles Wratten filters were employed for the purpose of excluding all light except that of a restricted wave-length range. Two sets of filters were used: the red filters were Wratten F, number 29; the green filters were Wratten Mercury Green, number 62. The transmissions of these filters are given in Table 1 as determined by one of us (D. B. J.).[13] For many settings of the wave-length the spectral transmission

TABLE 1

SPECTRAL TRANSMISSIONS OF FILTERS USED IN GLASSES FOR ADAPTATION

Wave Length in Mμ	Spectral Transmissions	
	Red Gelatine	Green Gelatine
405	<.0005	<.0006
436	<.00001	<.0001
492	<.0001	<.0009
500		<.0002
510		.0070
520		.086
530		.153
540		.121
550	<.00001	.059
560		.022
570		.0051
580	<.0003	<.0011
590	.0044	<.0001
600	.031	<.0001
610	.162	
620	.336	
630		
640	.695	
650		
660	.823	
670		
680	.858	
690		
700	.856	

was found to be too low to measure. In these cases it was recorded as less than an amount definitely measurable at that wave-length. From Table 1 it will be seen that the transmission of the red filter is virtually confined to wave-lengths greater than 590 mμ, while that of the green filter is virtually confined to wave-lengths between 510 and 570 mμ. In spite of the fact that no measurable transmission could be detected by the spectrophotometer for wave-lengths greater than 600 mμ, nevertheless the transmission of light by the green filter for that whole wave-length region is known to be appreciable because the filament of an incandescent lamp is visible as red through four thicknesses of the green filter combined with four thicknesses of the red filter.

[13] The entries in the table not preceded by the 'less than' sign represent averages of twenty determinations in each case. Stray light errors were avoided by the use of suitable filters; slit-width corrections were not made because they are known to be negligible. Measurements were made on the König-Martens spectrophotometer using the "tangent-cotangent" method of setting. See H. J. McNicholas, Equipment for routine spectral transmissions and reflection measurements, *B. S. J. Res.*, 1928, 1, 793–857. We wish to express our thanks to Mr. Irwin G. Priest, Chief of the Colorimetry Section, Bureau of Standards, for having made available the resources of the department for this study.

Two objects in this procedure were kept in view: (1) to choose filters which were highly selective, *i.e.*, practically excluding certain regions of the spectrum; (2) to observe for a sufficient length of time to insure complete adaptation if such a thing be possible with these filters. To insure sufficient time for complete adaptation two *O*s wore the glasses (either red or green) five hours, three *O*s four hours, two *O*s three hours, and several *O*s in periods ranging from 20 minutes to two hours. Since the results from all *O*s are in practical agreement tabulated reports are given for only one *O* each with the red and green filters in Tables 2 and 3, and the protocols of four *O*s concerning the general course of adaptation.

TABLE 2

REPORTS ON HUE, CHROMA, AND SATURATION OF COLOR SAMPLES DURING THE COURSE OF ADAPTATION WITH COLORED GLASSES WHOSE SPECTRAL TRANSMISSIONS ARE GIVEN IN TABLE 1

Wratten Red, F, No. 29

L, light; M, medium; D, dark; N, neutral or gray; S, saturated; R, red; G, green; Y, yellow; B, blue; O, orange. Small letters indicate minor component

Natural Color	Immediately with Glasses	After 1 Hour	After 2 Hours	After 3 Hours	After 4 Hours	After 5 Hours	Without Glasses
Stimulus 1 L RY or O	L YR	L YR	L YR	L YR	L YR	L yR	M YR
Stimulus 2 L gY	L YR	L YR	L YR	L YR	L YR	L yR	M YG
Stimulus 3 M yR	M R	L R	L R	L R	L YR	L YR	M yR
Stimulus 4 M B	D RB	D R	D N	D N	D N	D N	L B S
Stimulus 5 M bG	M R	M N	M N	M N	M N	M N	L G S
Stimulus 6 D rB	D RB	D RB	M RB	M bR	M bR	D bR	M B S
Stimulus 7 D N	D N	D N	D N	D N	D N	D N	D N
Stimulus 8 L N	L yR	L N	L yR	L yR	L yR	L yR	L N

Results. Comparison of the first column of Tables 2 and 3 with each of the other columns shows the changes that occur during the course of adaptation with colored glasses at intervals ranging from one to five hours and the after-effect of adaptation immediately upon taking off the glasses. The tabulated results with glasses substantiate the observations made with the color sphere. While there is some adaptation, as evidenced by reports of loss in saturation and occasionally almost complete disappearance of one component in complex hues, complete adaptation as evidenced by absolute loss of chroma did not take place with these glasses. It should be

TABLE 3

REPORTS ON HUE, CHROMA, AND SATURATION OF COLOR SAMPLES DURING COURSE OF ADAPTATION WITH COLORED GLASSES WHOSE SPECTRAL TRANSMISSIONS ARE GIVEN IN TABLE 1

Wratten Mercury Green, No. 62
(Symbols explained in Table 2)

Natural Color	Immediately with Glasses	After 1 Hour	After 2 Hours	After 3 Hours	After 4 Hours	After 5 Hours	Without Glasses
Stimulus 1 L O or RY.....	L N	M N	D gN	M gN	D gN	M N	L RY
Stimulus 2 L gY.....	yN	M N	ygN	L yN	L yN	L yN	L gY
Stimulus 3 M R S.....	D N	D N	D N	D N	D N	D N	M R S
Stimulus 4 L G.....	gyN	M oN	L oN	L oN	L oN	L oN	L bG
Stimulus 5 M B S.....	L N	M N	D N	D N	D N	D N	M B S
Stimulus 6 RB or rB.....	D rbN	D rbN	D rbN	D rbN	D rbN	D rbN	RB S
Stimulus 7 D N.....	D rbN	D rbN	D rbN	D rbN	D rbN	D rbN	D N
Stimulus 8 L N.....	L ygN	L yN	L gyN	L gyN	L gyN	L gyN	L N

noted that in those cases where a colored sample is reported as neutral after donning the glasses, as in Table 3, stimuli 3 and 5, the report of neutral was given immediately upon wearing the glasses, showing that the observed gray is a function of the transmissive properties of the glasses rather than of adaptation. A comparison of the first and last columns reveals that in spite of five hours' adaptation to a given color or colors, there is no evidence of the complementary after-image since the later descriptions are identical with or similar to the descriptions given before wearing the glasses. Momentary after-images were observable after discarding the glasses, but they were of short duration, extremely weak, and appeared only if the eyes were held fixed upon a surface favorable to their appearance. It is remarkable that the after-effects of such a long period of 'adaptation' with strongly selective glasses should have been so slight and transitory. Another fact of interest to be seen from the tables, is that papers normally appearing light gray or white appeared in the hues corresponding to the dominant wave-lengths transmitted

by the filters. On the other hand if the stimuli were either very dark or held so that the surface viewed was against the light, a hue complementary to that of the glasses was reported, as in the cases of stimuli 6 and 7 in Table 3.[14]

The following protocols describing the more general aspects of the course of adaptation give, perhaps, an even better idea of what happens, except for comparative purposes, when highly selective colored glasses are used:

Observer D. B. J.: After half an hour I looked at the sky (with green glasses) and snow illuminated by the sun. The sky looked yellowish-green at this time just as it did at the very beginning. Objects seemed to be non-selective; there were no chromatic differences noticeable and the gY of the illumination was not very noticeable. The bright portions of the visual field (windows, sky, *etc.*) were distinctly not neutral, but were Gy or gY though quite pale.

Observer H. H.: At first everything was green, a good green, but gradually there was a loss in chroma until objects had only a tinge of green. All of this occurred within five minutes after putting on the glasses. But some green was visible at all times, from beginning to end of the observation period (five hours). The brightness toward which objects tended under strong illumination was a bright or light gray, not a neutral gray.

Observer M. S.: Everything was tinted with red all the time the glasses were worn. But as time went on, the yellow component came out more and more—the pinks got to be yellowish pinks. Dark colors became brownish. Natural colors were absent from the time the glasses were worn. Surfaces normally yielding saturated colors seemed to be gray with a red tint over them, which was even true of the grass.

Observer F. W.: Sun and sunlight looked quite red after 30 minutes of donning the goggles. White paper looks yellow.

[14] Observer H. H. noticed that if the papers were held in a certain way, *i.e.*, regarded from the reverse side from which they were illuminated, they were covered by a film of color complementary to that of the dominant hue of the glasses. This is an example of the 'dimming effect' wherein a decrease in the intensity of light (provided it does not reach zero or nearly zero, as will be shown later) after adaptation to a color arouses the negative after-image.

Grass outdoors looks dark gray with reddish, filmy bulk over it. Green desk blotter looked dark gray also. One hour after donning goggles the reddish tinge was noticeably decreased; it is more of a reddish-orange. There is some interplay, a moving back and forth, between orange-yellow and red. The sunlight out of doors became distinctly yellow but there was some fluctuation of red—momentary waves of red.[15] The two are frequently present simultaneously; part of the field was tinged with red, part with yellow.

It can easily be gathered from these protocols that objects did not assume their natural appearance with the glasses used in these experiments. To say that objects will assume a natural appearance implies that the *O* will be able to distinguish color differences for which there are no stimuli, since the filters practically exclude light of some wave-lengths. It is possible, however, to wear colored glasses which will verify the usual statements. If such glasses are measured spectrophotometrically it will be found that they are only weakly selective, *i.e.*, a complete spectrum will be seen, merely reduced in intensity in certain regions. The glasses sold commercially are usually of this type.

We are now in a position to explain why complete adaptation occurs with some colored glasses and not with others. If the glasses are only mildly selective, that is, merely transmit more light of certain wave-lengths than others, a given color may predominate (the color of the glasses) but colors corresponding to all wave-lengths are present. Adaptation takes place in sufficient amount to reduce the slight predominance given by the glasses to the colors corresponding to certain wave-lengths. Thus objects may appear more or less natural after adaptation because light of all wave-lengths is transmitted about equally, the slight inequality in transmission being partly compensated through adaptation. But if highly selective glasses are worn light of certain wave-lengths is practically excluded from the eye and hence differences depending

[15] Compare this alternation of red and yellow with that described in the sphere experiments above. It is needless to point out that this *O* was given no intimation of the results with the sphere which were obtained six years earlier, the experiments with glasses having been performed in 1930.

upon such light cannot be seen no matter to what degree adaptation proceeds. Adaptation cannot supply what is not present. Our observations show that in such cases adaptation does not even succeed in cancelling the dominant color component to give neutral gray as demanded by the Hering theory. Highly selective glasses give red and highly selective green glasses give green no matter how long the glasses are worn—and from the red and green glasses used in these experiments we judge that a similar conclusion holds for glasses of any color.[16]

One fact remains to be mentioned which again emphasizes the necessity for specifying the conditions under which various effects of adaptation are to be expected. We have seen that the after-image, whose presence should be observable (according to the Hering theory) as soon as exposure to the stimulus has ceased, was either of extremely short duration or failed to be observed unless the eyes were held stationary upon a proper surface. Special observation showed that if the glasses were removed in a completely dark room, no after-image appeared in spite of the fact that the Hering conditions were being obeyed. Troland has reported the same phenomenon under slightly different conditions. If some light is present, a momentary after-image appears, complementary in hue to the dominant color of the glasses.[17] We shall revert to this fact again in our theoretical discussion below.

We may summarize the results of this second set of experiments with highly selective glasses as follows:

(1) Even after five hours the brighter portions of the visual field retained the colors corresponding to the wave-lengths of the light transmitted by the glasses.

[16] This fact may be of some importance to the optical industry. It might, for example, be important to determine how far adaptation can reduce the dominant color of a given glass to render a natural appearance to objects. The whole question of visual tolerance deserves scientific investigation.

[17] If one does not go into a dark room immediately after exposure but allows the negative after-image to "get a start" it will be seen after entering the dark. We do not deny that under some conditions after-images develop in the dark immediately after exposure to stimulation. The well-known phenomenon of the flight of colors shows that after-images may get started with apparently no light present. But we are dealing with entirely different conditions in the flight of colors and adaptation with colored glasses.

(2) Objects showed no tendency to appear "natural"; they appeared largely as pale colors. The lighter colors had approximately the hue of the glasses, the darker colors were either completely neutral or were complementary in hue to the glasses.[18]

(3) Some adaptation occurred, notably at the beginning, during which there was more or less loss in saturation in the dominant hue and often a shifting toward another component in accordance with well-established laws.

(4) Weakly selective glasses that do not impose too much of a strain upon the mechanism of adaptation which transmit some red, some green and some blue light may be used to verify the statements usually made regarding adaptation but which do not hold for highly selective glasses.

(5) The negative after-image fails to appear unless some light is present, and is then of extremely short duration. There is a tendency, however, for colors complementary to the color of the glasses to be accentuated for a short period following removal of the glasses.

Discussion of Results

Our results substantiate the fact that complete adaptation does not occur except, as Troland has said, under very special conditions involving stationary fixation. The rôle of eye-movements has been in dispute due to the fact that until now no method has been devised for permitting eye-movements while stimulation was kept constant. Our use of the 'adaptation sphere' has enabled us to isolate eye-movements by maintaining constant stimulation through the elimination of pupillary changes and local differences in intensity of light falling upon the retina. We must conclude that eye-movements alone are sufficient to regenerate the retina so that complete adaptation is never reached even with constant stimulation of the retinal elements responsible for color vision. Whether eye-movements facilitate regeneration by facilitating the streaming phenomena, as Ferree has claimed, or in some other fashion, we do not pretend to say at present. We doubt,

[18] For an exception see footnote 14.

however, if Ferree's supposition holds under our conditions because here the whole retina is stimulated and it would appear from Ferree's discussion of the streaming phenomena that for the streaming to be effective different parts of the retina should be stimulated unequally since the streaming takes place from periphery to center, or from a region of lesser stimulation to one of greater stimulation. On the other hand, we do not deny that other factors besides eye-movements are responsible for restoring the retina.[19] Our results in no way imply that pupillary changes, the streaming phenomena, changes in the intensity of the stimuli, and possibly other factors do not aid in restoration. They do show, however, that eye-movements alone can bring this about, without differences in intensity of the light reaching the retina, pupillary changes, the streaming phenomena, or freeing the retina from stimulation.

The usual view of the function of eye-movements in adaptation has been that through their agency different parts of the retina receive different increments of stimulation, lesser stimulation allowing for recovery. When colored glasses are worn and the eyes are allowed to move, this view of the function of eye-movements is undoubtedly correct. In the sphere, however, all parts of the retina were stimulated equally at all times, and hence we must attribute to eye-movements a regenerative rôle when all other means are absent. Whether eye-movements arrest adaptation through their effects upon circulation, distribution of lymph, nutritive or other processes we do not venture to decide. It is probable that all retinal processes are affected by eye-movements, some more, others

[19] The factors influencing photopic adaptation are many. Here is a by no means exhaustive list gathered from the literature and our own experience: (1) composition and intensity of the stimuli; (2) state of the retina previous to and during stimulation; (3) stationary *vs.* moving fixation; (4) local *vs.* general stimulation of the retina; (5) portion of the retina stimulated, if local stimulation is used; (6) duration of stimulation; (7) size of the pupil and changes in size during observation; (8) absolute intensity of the stimuli; (9) retinal rivalry if monocular observations are made; (10) condition of the unstimulated eye if monocular stimulation is used; (11) transmissive properties of filters or glasses if these are used; (12) conditions of observation after exposure if after-image phenomena are investigated. It will be seen that while we emphasize the rôle of eye-movements in this study we do not believe it to be the sole factor influencing adaptation by any means.

less. Their effect upon adaptation is pronounced, for in the moving eye, in spite of constant conditions of stimulation, only a small amount of adaptation occurs; there is never complete loss of chroma.

Adaptation with colored glasses that are highly selective results in some loss in saturation and brightness and is a reduction process primarily, except in so far as adaptation to a given color enhances its complementary or causes a shift toward another region of the spectrum, as in Voeste's experiments. But if colored glasses exclude light of certain wavelengths, adaptation cannot supply them. Hence the statement that if colored glasses are worn for a sufficiently long time "objects are seen sooner or later in their natural colors" is true only for glasses that give a predominance to some portions of the spectrum, but exclude none. The experiments of Maria Bokowa in which complete extinction of the color of the glasses was reported can be explained by the fact that her cuprous oxide glasses were not highly absorptive and hence only mildly selective. Adaptation carries some reduction in chroma with it even with moving fixation, as we found in both our sphere experiments and with colored glasses. In the latter case eye-movements also bring changes in intensity and composition of the light which finally reaches the eyes.

We wish to emphasize the fact that generalizations concerning the effects of colored glasses whether made respecting the course of adaptation, the enhancement of certain colors, or the general appearance of objects mean nothing unless stated with reference to the spectral transmissions of the glasses employed. As usually made such generalizations are not only untrue for *all* glasses but are positively misleading in implying that psychological processes will supply a distinction between two color stimuli which are physically identical.

Earlier in the paper it was remarked that statements regarding adaptation and its effects seemed to spring as much from the Hering theory as from demonstrated fact since current generalizations fit better with the Hering assumptions than with crucial facts easily found in the literature. It is

not our intention to make an assault upon the Hering theory by marshalling all the facts that can be found against it. At this point we wish merely to recapitulate our findings and their bearing upon the Hering theory in so far as there is a connection between the theory and the facts of adaptation.

According to Hering's own statement we should expect the metabolic processes in the retina to reach a state of equilibrium after a time if the intensity of light falling upon the retinal elements is constant. Our failure to achieve complete adaptation and the Hering mid-gray visual quality under our sphere conditions, as well as Troland's failure to do so even with the stationary eye and saturated stimuli, shows that the concept of a Hering equilibrium and its associated gray quality are far from being realized under as wide a variety of conditions as proponents of the theory would have us suppose. Complete adaptation appears to be a rather extraordinary phenomenon, though easily obtainable under certain restricted and artificial conditions. The Hering mid- or neutral-gray would appear to be an even more extraordinary phenomenon, appearing under even more restricted conditions than those required for the disappearance of chroma since the gray reached in complete adaptation is not always the Hering mid-gray. In short, the effects of adaptation demanded by the Hering theory, far from being found under all conditions, seem to appear only with stationary fixation and stimuli of medium saturations and intensities.

The Hering theory further requires that after stimulation of one color process there shall be a reaction of the antagonistic color process accompanied by the negative after-image. In the second set of experiments when observation was made in the dark room after taking off the colored glasses, although the eyes were relieved from stimulation of the color of the glasses, no after-image appeared. Hering himself has suggested that some light may be necessary for the antagonistic process that reverses a given retinal process. If this is true the concept of autonomous regeneration accompanied by the complementary color falls to the ground, since the mechanism of regeneration is supposed to function when the eye is re-

lieved from stimulation. That regeneration does take place when the eye is relieved from stimulation, and also during stimulation, we do not deny. But the failure of the negative after-image to appear under these conditions calls into question the Hering explanation of the negative after-image. It is possible that the antagonistic metabolic process may have been present when no after-image was observable but it should be remembered that one of the touted merits of the Hering theory is its supposed ability to explain the negative after-image. It may well be that after-images are a result of sensitization to certain (complementary?) wave-lengths after fatigue to others as postulated by Professor Frank Allen,[20] rather than an effect of the self-activity of the retina as Hering claimed. We shall not try to give an alternative explanation of all the phenomena of vision since it is not our purpose to perpetrate another visual theory upon a subject already overburdened with theories. We wish to point out that when the generalizations concerning adaptation are corrected they will sound less like retranslations of certain facts into the Hering theory and will be more in harmony with all of the observed data and well-established physical principles which are too often lost sight of in psychological discussions of visual phenomena.

Summary and Conclusions

Eye-movements alone, all other factors held constant, are sufficient to arrest the course of adaptation so that complete adaptation with total loss of chroma does not occur. With the moving eye and general stimulation the process of adaptation is confined to a brief interval (not more than a few minutes) after which no further change is discernible. Rhythmical effects, at present unaccounted for, may be observed both under our 'sphere' conditions and with colored glasses.

Complete adaptation with colored glasses so that objects resume their natural appearance occurs only with weakly selective glasses as proved by the use of highly selective glasses whose dominant color persists in vision no matter how long

[20] F. Allen, On reflex sensations, *J. O. S. A. and R. S. I.*, 1923, 7, 583–626, 913–942.

they are worn. The negative after-image fails to appear after several hours of adaptation with colored glasses if observations are made immediately in a dark room or if the eyes are moved in the light.

Our results add fresh evidence against the Hering theory of vision with its concepts of color-pairs, antagonistic processes, and a mid-gray quality as the end-result of adaptation.

The laws of adaptation as classically stated should be either revised to include crucial facts of observation and well-known facts of physics, or stated with reference to the particular conditions under which they are valid. These conditions are so limited and artificial that they fail to take into account features of normal vision like eye-movements and general stimulation which should receive consideration.

(Manuscript received August 16, 1931)

THE RESPONSIVENESS OF THE BLIND SPOT

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I. Introduction

The blind spot on the human retina under ordinary conditions is insensitive to stimulation, although we are seldom aware of this defect.

Even when we close one of our eyes and look upon a uniform surface we do not notice any difference between that part of the surface which stimulates the blind spot and the rest of the visual field; the total visual field is homogeneous. This same effect of filling-in is produced no matter what the color or brightness of the surface. Obviously our eye responds to a field as a whole. The places of nonstimulation like the space occupied by the entrance of the optic nerve and the spaces between the rods and cones, are completed for us.

This completory (*ergänzende*) or totalizing (*totalisierende*) process was shown very nicely in some work by Fuchs [1] and Poppelreuter [2] with hemianopic patients who were found to recognize and complete familiar figures, parts of which fell upon the nonsensitive parts of the retina. In view of these readily observable facts we must bear in mind in our study of the blind spot that it does not give us under ordinary conditions the same impression as the mere absence of stimulation.

Although unobservable when the eye is directed at a uniform field, a blindness of the blind spot is noticeable if we fixate a point on a white piece of cardboard and move a small black object in and out of the region of the cardboard which corresponds to the blind spot. This means of determining

[1] W. Fuchs, *Untersuchungen über das Sehen der Hemianopiker und Hemiamblopiker*, in "Psychologische Analysen hirnpathologischer Fälle" by A. Gelb and K. Goldstein, Barth, 1920, 419–561.

[2] W. Poppelreuter, *Die psychischen Schädigungen durch Kopfschuss usf.* Bd. I: Die Störungen der niederen und Höheren Sehleistungen durch Verletzungen des Okzipitalhirns. Leipzig, Voss, 1917, pp. 149 ff.

the location of the blind spot by the absence of an object is familiar to most people.

More unfamiliar and yet known for some time is the phenomenon of the positive response on the blind spot when conditions of maximum brightness contrast in favor of the blind spot are secured. For instance, if an observer fixates with one eye a dim fixation light on a dark background, he will experience a light sensation when a light which is well above the brightness threshold for the surrounding sensitive region and which contrasts sharply with the background is thrown upon his blind spot. The subject will locate this light sensation on the stimulus surface in the direction of the projection of his blind spot and at the proper distance away.

Under the last mentioned conditions two factors would seem to be operating; viz., the physical irradiation of light with increased intensity, and the contrast phenomenon. With very low illuminations the region of the entrance of the optic nerve is blind regardless of maximum contrast conditions. With higher illuminations the blind spot seems to become positively sensitive although the fineness of positive localizability and visibility upon the blind spot seems to depend, as we shall indicate later in the paper, upon a proper intensity of illumination of the blind spot and an absence of direct stimulation upon the surrounding regions, or in other words, upon optimal conditions for contrast.

Under ordinary daylight observation the totalizing phenomenon operates to fill the gap, whereas under thes pecial laboratory conditions the irradiation of light from the blind spot to surrounding sensitive but directly unstimulated regions operates to enable us to distinguish the blind spot from the rest of the visual field. These two factors apparently work integratively with one another as we shall show later.

It will be the purpose of this paper to present some experimental results which throw further light upon the functioning of the blind spot and in particular upon the functioning of the totalizing and contrast phenomena.

Apparatus. All of these experiments were carried out in a dark room. At a distance of 131 cm or 4 meters, depending on the nature of the particular experiment under investigation, the *O* faced a black cardboard background, 114 cm by 70 cm in

size. The *O* with his left eye fixated a pinpoint of light emanating from a tiny hole in a black painted 3 volt flashlight bulb.

Off to the left, around the area corresponding to the projection of the blind spot, we made an opening somewhat larger than the largest size of the blind spot at the greatest distance away. By a sliding groove arrangement we were able to insert cardboard patterns used as stimuli for the blind spot. The *O*'s head was held fast in a head clamp fastened to a laboratory table.

Two different stimulus lights were used behind the cardboard patterns. In those experiments in which a powerful light was required and in which the qualitative factor was not crucial we used an ordinary light box housing a 200 watt bulb. A ground glass screen was placed over the face of the light box between the light and the pattern cut from cardboard. Variation in intensity was secured by means of rheostats or filters.

For the lower intensity light we used a Nagel adaptometer, which in addition to giving a white light at all intensities has the advantage of yielding an exact calibration of the light intensity used. This instrument was particularly valuable for the investigation of visual quality at near liminal values and also for the quantitative study of the brightness threshold of the blind spot. At full intensity the adaptometer provides a light of only .13 candle power, and the portion of the total candle power used in any given experiment can be readily determined by calculating the proportion the given dial reading bears to the dial reading at full intensity.

Observers. Two groups of observers were used. In our preliminary investigations upon quality, in the determination of the limen, and in some of the work upon the process of adaptation our observers were: S. H. Bartley, E. Newman, F. T. Perkins, R. Greenberg, G. Smith, K. Seltsam and Dr. B. Morrison. Our observers during the fall of 1930 were: S. H. Bartley, R. Brigden, E. Patton, and Drs. B. Morrison and D. M. Purdy. All observers were asked to guard against eye movements and to report anything that might occur. After some practice they had little trouble in focussing upon the fixation light and attending to the stimulus light in the blind spot. Care was taken to eliminate fatigue by interspersing frequent rest periods between observations.

Technique. The mapping of the blind spot was done with the room dimly lighted. The subject fixated the fixation point with his left eye. The experimenter then explored the limits of the projected areas of the blind spot with a white tipped probe. The points of disappearance (thus giving the smallest area, so that our stimuli would be well within the limits of the blind spot) were exactly marked along eight meridians on the black cardboard. By means of the interchangeable patterns we were thus able to secure quite accurate maps of the blind spot for each subject. At 131 cm the average blind spot of our subjects represented a figure approximately 14 cm in diameter along the vertical axis and 10 cm along the horizontal axis.

With the exception of the instances which we have noted explicitly the *O*s worked without knowledge. We consider this precaution of considerable importance, especially in experiments on form vision. The experimenters were careful to refrain from leading or suggestive questions, particularly in matters of terminology, although they did their utmost to encourage a complete report.

II. Experimental

A. Qualitative Description of Light on the Blind Spot

Source of light: the Nagel adaptometer. Distance of O from fixation light: 131 cm. A 1 cm circular light at a near liminal intensity was exposed. O observed it for a short time. The light was switched off, and O made his descriptive report.

At an intensity sufficient to be just barely perceptible none of our Os (Ba, Br, M, Pa, and Pu) reported a bright central core. All of them described the light as being a dim, homogeneous light. Some typical reports: 'a cloud-like object . . . a big expanse of grey . . . *no core*' (Pu); 'a blur of light . . . a very dim, diffuse haziness' (M); 'very indefinite haze . . . no core, simply a grey patch' (Ba). When the intensity of the light was increased to a point considerably above the limen, or when the increasing sensitivity with adaptation caused what had been a near liminal value to pass well above the limen, the Os tended to report the light as being brighter towards the center.

In order to investigate the nature of this so-called 'core,' the experimenter started the adaptometer light at a point well below the limen and then increased the intensity of the light until the full intensity was exposed on the blind spot. All of our Os except one, Pu, saw the light as beginning in an indefinite, undifferentiated haziness, then increasing in brightness in the center, until at the end, that is, at maximal intensity, they saw a definite core with a halo. Ba's report is typical: "The light increased slowly in intensity—hazy, then a core appeared."

Pu failed at any time during this portion of the experiment to get a core even at the maximal brightness, but this is probably due to the fact that since the process of adaptation had not advanced very far, his sensitivity was not great enough to obtain this effect; in other words, the limen itself was at this stage of adaptation too high on the scale to allow for sufficient increase of intensity to obtain the 'core.' This belief is borne out by the fact that later during the experiment he did obtain a 'core' at intensities well above the limen.

At the highest intensities our Os reported that the bright core gave way to a blinding homogeneous glare.

An additional investigation was carried out on two of the observers: A near liminal light was exposed on the blind spot. Then this light was decreased in intensity to the near liminal value for the sensitive retina, and the O was asked to observe the stimulus light directly and to compare it with the impression of the light as seen on the blind spot. By thus comparing a near liminal light on the blind spot with near liminal light for direct vision a fair comparison of the relative qualities was obtained. Our results show that if there are any qualitative differences at all under such circumstances they must be very slight. Our Os failed to report any qualitative difference whatsoever, describing both alike. M reports: "Both are of a bluish ethereal cast." Pu says there is 'no color except greyishness in both cases.' These reports indicate that the bluish grey color perceived at the limen on the blind spot is no different from the color perceived at the limen on the retina proper.

In order to determine to some extent what effect variation in the intensity of the stimulus would have upon the perception of light within the blind spot, we arranged a series of experiments in which the stimulus light at an intensity just above the threshold was permitted to fall upon the blind spot of the O; and the light was immediately increased in intensity, as the O held his fixation, to a point much above the threshold; *i.e.*, to an intensity ten times, say, the value of the light at first. By using the adaptometer we were thus able to vary the intensity at a fairly uniform rate.

Under these conditions the Os were asked to observe and to report changes that took place. With this increasing intensity all of the Os agreed that the light, which at the beginning near liminal value was of the vague, hazy, and diffuse greyish color we have described as being characteristic of the light in the lowest perceptible ranges, increased in brightness until it appeared to have a 'core' in the center. These results show definitely that the quality of 'coredness' does not appear when the blind spot is stimulated with lights of just liminal

value, but that the perception of 'core' comes, just as it does on the sensitive part of the retina, at an intensity considerably above the liminal value.[3]

B. The Limen

After an adaptation period of 45 minutes the O faced the adaptometer at a distance of 131 cm from the fixation point. A circular opening 1 cm in diameter permitted the light from the adaptometer to fall upon the center of the blind spot. A cardboard was let down and raised in front of the opening to give a short exposure. This short exposure was necessary as it was found that a very weak stimulus rapidly adapts out. If the O was able to report correctly and with consistency when the light was thus exposed at a given intensity, the light intensity was decreased and the process repeated. In this way a value was reached at which the light could just barely be perceived.

The following list of limen determinations for nine Os must be considered in terms of the distance of the light from the O, 131 cm, and the size of the exposed light, a circle of 1 cm diameter:

Ba	.0112 c.p.
D	.0195
G	.0104
M	.0325
N	.0130
Pe	.0091
Se	.0130
Sm	.0104
W	.0130

With the exception of the limen for M these limens show a fairly consistent grouping around the mode, .013 c.p. The remarkable thing about the limens determined is that such a very small amount of light formed a sufficient stimulus. Even in the case of the limen for M, in which we have an intensity more than twice that of the mode, it is hard to understand how, in terms of the assumption of the sensibility of the optic nerve trunk, at a distance of 131 cm, M could get adequate stimulation from a light of .0325 c.p. Actually

[3] A. Feinberg, *Experimentelle Untersuchungen über die Wahrnehmung im Gebiet des blinden Flecks, Psychol. Forsch.*, 7, 1926, 26, reports the same result.

in a majority of cases the light was less than half that intense. On the basis of the sensibility of the optic nerve itself this would mean that we would have a situation in which the nerve fiber, which is commonly regarded as functioning primarily for purposes of conduction, could claim for itself the main property of an end organ—a lowered threshold for reception of stimulation of a given modality. If it were true that the nerve fiber itself were responding directly to light this would introduce a new variable into the conditions governing stimulability of nervous tissue, one that is hardly substantiated by the present status of the problem, since all stimulation of nervous tissue by means of light is detectable only in case of lights of high intensity, much greater than the intensities used in these experiments.

C. *Central versus Peripheral Sensitivity on the Blind Spot*

Important insight into the question regarding the exact mechanism of sensibility is indicated by the results of our experiments upon the relative sensitivity of the center of the blind spot and the edge of the blind spot. A gradient of sensitivity with its maximum at the edge and its minimum at the center of the blind spot would follow logically from the diffusion hypothesis, but it is difficult to conceive how, upon the assumption of optic nerve sensitivity, one could ever explain an increase in sensitivity as the light is moved toward the edge. Indeed, it would seem more likely to suppose that upon the terms of these assumptions the center of the blind spot would be more sensitive.

In order to determine the relative sensitivities of the edge and the center of the blind spot, we conducted a series of experiments to obtain the comparative limens of the two. First we measured the intensity necessary for light to be perceived at the center of the blind spot. Then, by moving the fixation light to one side so that the stimulus light fell upon a point about 2 cm from the edge of the blind spot as mapped, we were able to determine the limen at a point near the edge. We could be sure that we were still stimulating the area well within the blind spot because of the fact that our maps of the

blind spot were taken by marking the points at which the light disappeared upon bringing our probe in, rather than the points of appearance when the probes were moved out, and because the distance from the edge which we selected, 2 cm, in itself gave us a liberal margin of safety.

Observer	Limen for Center	Limen for Edge
Ba	.0112 c.p.	.0056 c.p.
Br	.0164	.0051
D	.0173	.0060
Pu	.0103	.0043
W	.0130	.0026

These results clearly prove that the edge of the blind spot is more sensitive than the center.[4] This can be adequately explained according to the theory that the blind spot's sensitivity is due to an irradiation which stimulates receptors of the adjacent retinal field. As discussed later these receptors surrounding the blind spot have peculiar spatial properties. How these results can be explained in terms of the theory of direct optic nerve stimulation is hard to see.

D. Adaptation Inside and Outside of the Blind Spot

Limen determinations were made every ten minutes following the procedure outlined in Section B. Because of the length of time required to ascertain the limen in each individual reading, it was impracticable to determine the adaptation of the blind spot and the retina at the same time. It was therefore necessary to make a complete record of adaptation for the blind spot, and then, at another time to make a similar record for the retina outside.

In order to determine whether there is any real difference between the rate of adaptation on the blind spot and the rate of adaptation on the retina we made comparative adaptation records for them. Our findings fail to substantiate in any way the assumption that the adaptation of the retina and of the blind spot is essentially different in any way, except that the limens in the case of the blind spot were considerably

[4] Our findings are supported by similar results from work by C. F. Scofield, Perception in the region of the optic disk, *Amer. Jour. Psychol.*, 42, 1930, 213–234, and by work done in the psychological laboratory of the University of Giessen and reported by Koffka in *Brit. Jour. Psychol.*, 14, 1924, 273.

higher. Adaptation proceeded at a fairly equal rate in the two and terminated at approximately the same time in both. This concomitant change in sensitivity during the process of adaptation argues very strongly in favor of the diffusion hypothesis.

The following tables of liminal values at ten minute intervals for three subjects holds only for the given distance, 131 cm, and for the given area, a circle of a diameter of 1 cm.

Minutes	Subject Ba		Subject N		Subject D	
	Bl. Spot	Retina	Bl. Spot	Retina	Bl. Spot	Retina
0		.000259		.000173		.000133
10	.0822	.000069	.0520	.000086	.0866	.000055
20	.0303	.000051	.0104	.000051	.0554	.000033
30	.0129	.000034	.0130	.000043	.0259	.000022
40	.0086	.000043	.0130	.000034	.0181	.000013
50	.0086	.000034	.0130	.000034	.0155	.000013
60	.0086	.000034	.0130	.000034	.0138	.000013

It will be noticed that with no adaptation we were unable to get any report as to the limen on the blind spot. Since the adaptometer would not give us a supra-liminal light under these conditions we were forced to omit the limens.

The above tables show that in both cases the greater part of the process of adaptation goes on during the first thirty minutes. The relative rate of adaptation seems to correspond closely during all stages of the process. While these results are not extensive they suggest that adaptation within the blind spot is the same sort of process that we have in the sensitive portion of the retina. More than that, they point to the fact that the "adaptation of the blind spot" is simply the process of adaptation of the retinal field itself.

E. Form and Color Vision on the Blind Spot

1. Form Vision in the Center of the Blind Spot

Observers were stimulated upon their blind spots with forms of a triangle, a cross, a half circle, an escutcheon-shaped figure, an octagon and a circle. (Distance from stimulus, 131 cm. Source of stimulus 200 watt lamp.) Precautions were taken to ensure that the pattern fell at approximately

the center of the blind spot. Characteristic reports of observers were as follows: Triangle—M: "A big splash of light"; Br: "Looks circular"; Pu: "Glow with haze around"; B: "A bright patch, fairly concentrated, no form." Cross—M: "No definite outline, appears elliptical"; Br: "Like an oval, very near circular"; B: "A jumbly patch of light." Variations of the length of the exposure from the briefest intervals to comparatively long intervals failed to give the impression of form.

The extracts of protocols reported above indicate the type of experience obtained by all observers. In no case under these conditions did an observer come anywhere near to an adequate description of the form of the figure exposed. Possibly one reason for our failure to obtain any indication of form perception on the blind spot lay in the fact that we were careful to make the edges of our forms fall well within the limits of the blind spot and to give our observers no indication of the nature of the figures beforehand.

2. Form Vision toward the Periphery

By moving our fixation point toward the figure which was used to stimulate the blind spot we were able to study the appearance of the stimulus on the periphery of the blind spot, and its appearance when partly out and partly in the blind spot. As the boundaries of the blind spot are not distinct it is impossible to tell exactly what part of the stimulus is on the blind spot and what part out, but by checking the form of the blind spot each time before beginning a period of investigation we were able to make fairly accurate estimations of the position of the stimulus figure with reference to the blind spot area. The reports for the cross figure ran as follows: *Near edge yet within blind spot.* M—"Looked round, quite blurred." *Figure partly in and partly out of blind spot.* "Some form, but not sure what, like a flat surface of light broken up with light streaks." *Figure almost entirely out of the blind spot.* "Might have been a cross." Pu—*Near edge yet within blind spot.* "Couldn't describe—not well marked off—apparently symmetrical." *Figure partly in and partly out of blind spot.* "Conscious of form, not symmetrical, but

couldn't describe it." *Figure almost entirely out of the blind spot.* "Pattern not symmetrical—horizontal axis longer—a sharp corner at right—something like a trapezoid." Br—*Near edge yet within blind spot.* "Didn't see well—more form than before." *Figure partly in and partly out of blind spot.* "Like an inverted club, not an even field, dimmer around edges—it is not circular. Form seems clearer." *Figure almost entirely out of blind spot.* "A teaser, thought I saw form but couldn't quite—like four leaves on a club—sort of four leaf clover design." Ba—*Near edge yet within blind spot.* "Looked like three superimposed discs." *Partly in and partly out of blind spot.* "Looked like an inverted triangle with a broad base." *Figure almost entirely out of blind spot.* "Looked like a disc with a thin rectangle—almost like a bright cross."

The same kind of reports were given for the other kinds of figures. As the different forms approached the edge of the blind spot they were perceived more clearly and accurately by the observers. This fact was corroborated by the observers when they were informed of the nature of the experiment and asked to verify the conclusions.

3. Perception of Concentric Circles

One of the clever suggestions made in Helson's [5] investigation was the use of a circle inside of a concentric ring and separated from it by a dark ring. In order to make the inner circle and the neighboring ring as qualitatively different as possible the inner circle was made one color, and the outer ring a different color (by the use of gelatine filters). One light source illuminated the inner circle, and another light source illuminated the outer ring. By the use of resistances the brightness of the two rings could be equated. When either the center light or the peripheral light was more intense the Os always saw the whole figure in terms of the brightest color. Likewise when the relative brightness of the colors was changed gradually so that the light which was brightest was gradually dimmed until the other light became the brightest, the Os always reported that a fusion phenomenon took place

[5] *Amer. Jour. Psychol.*, 41, 1929, 382.

during the intermediate stages. Sample reports were as follows: M—"Red at first and then faded into blue. Fading diffuse and not clear cut." Pu: "Looked blue at first, then red. It was homogeneous red and blue alternating. Never see the colors at the same time." Br—"Red seems to come in from edges, blue to fade out. Sometimes a sort of purplish red. Uniform all over the surface." Ba—"Bluish tinge, then red came covering over it—overlaying it. It was as if particles are mixed, making a sort of purplish tinge."

A definite impression of one circle of light within another was never obtained by our Os.

While working with concentric circles it occurred to us to try to determine the effect of enlarging and diminishing a ring of light so as to make it fall completely within, partly within, and completely without the blind spot. We found that the nature of the sensory quality upon the blind spot was determined in every case by the nature of the stimulation of those receptors nearest the edge of the blind spot. When the inside edge of the ring fell outside of the blind spot so that the adjacent receptors were not directly stimulated by light the blind spot appeared dark. When the inside edge of the lighted ring fell within the blind spot and the outside edge stimulated the sensitive edges of the blind spot, the blind spot appeared light. By slightly enlarging and then slightly decreasing the size of the ring, the blind spot could be made to appear alternately dark and light, showing in a very simple way how the sensory data from receptors adjacent to the blind spot are totalized and referred to the blind spot.

III. Theory and Conclusions

In conclusion let us take up some of the different theories which have been advanced to explain the responsiveness of the blind spot to light stimulation: [6]

[6] See H. Helson, The Effects of Direct Stimulation of the Blind Spot, *Amer. Jour. Psychol.*, 41, 1929, 393–397 for a different viewpoint as regards these three theories.

While our investigation approximates in many ways to Dr. Helson's work upon the same subject it is in no way a duplication of it. Helson has framed experiments and marshalled his data to overthrow what appears to him as an utterly inadequate 'irradiation theory.' He will have none of the old and would lead his reader to believe that irradiation and other older theories are utterly at variance with his two suggested

A. The assumption that end organs in the blind spot mediate the perception of stimuli.

1. Under the conditions of our investigation our Os were unable to distinguish forms upon the blind spot. What few vague impressions were reported could be explained just as well by differentiations of brightness upon the surrounding receptors.

2. Under our experimental conditions our Os were never able to see one light stimulus inside of another on the blind spot. This fact, together with the observation mentioned above, argues against the presence of receptors in the blind spot. It indicates that either the hypothetical receptors in the blind spot do not exist, or else, if they do exist, they are comparatively insensitive and of no consequence under ordinary illumination. The well recognized totalizing phenomenon would seem to be vastly more important in explaining the filling-in process under ordinary conditions of illumination than would the receptors in the blind spot.

3. Another significant argument against this theory is the fact that no neurologist or ophthalmologist, so far as the authors can determine, has ever discovered any receptors in the blind spot. Much careful work has been done in studying the normal and diseased retina and yet no one has discovered as yet any traces of nerve endings. The accompanying illustration is a typical reproduction of specimen cross-sections of the blind spot. It can be seen quite clearly by a study of this diagram that the rods and cones cease abruptly at a certain distance from the center of the optic nerve entrance.

B. The assumption that the optic nerve trunk is responsive to light.

While there is no definite proof of the human optic nerve trunk being responsive to light, there is no good proof to the contrary. Thus this hypothesis is scientifically plausible until further investigations bearing specifically on this point

assumptions. Although they differ radically we believe that Helson's results and ours are not mutually exclusive. While admitting the influence of physical irradiation we have organized our results around the concepts of the totalizing perception and of the well known contrast phenomenon and have thus achieved a picture which we believe enables us to synthetize the significant facts, both old and new.

are made. It is possible that the optic nerve is stimulated by light of high intensity, but any responsiveness of this sort has probably little significance as regards the interpretation of our observations, many of which were made with lights of very low values. The perceptual phenomena are more satis-

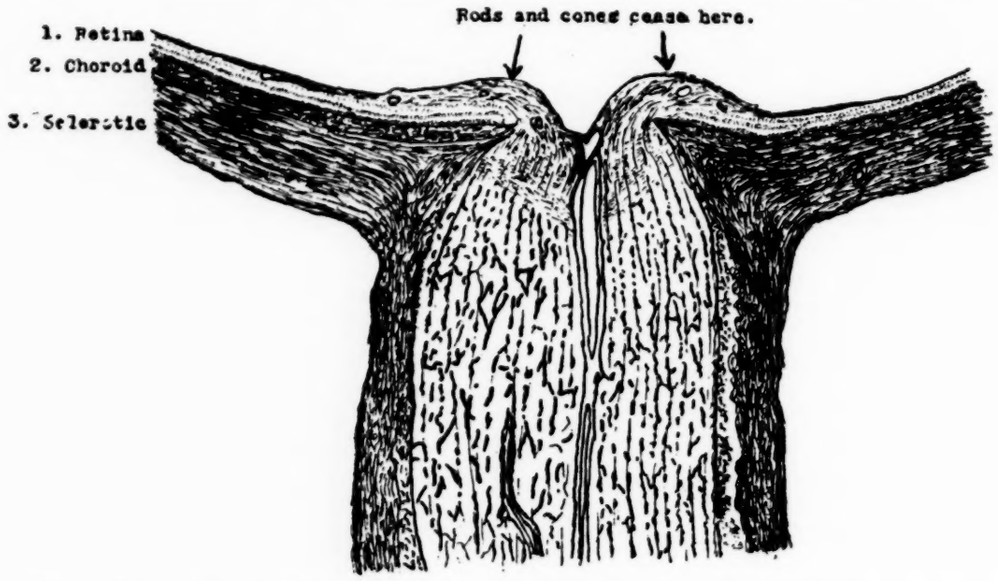

FIG. 1. Drawing made from normal cross-section specimen of the human retina.

factorily accounted for in terms of the recognized totalizing functioning of the surrounding receptors together with the enhancement of formedness by optimal contrast conditions.

C. The assumption that the surrounding retinal elements function integratively to give the positive impression of light in the blind spot.

Since both our common sense observations mentioned in the Introduction, and the results of our experimentation point to the integrative functioning of the receptors in the vicinity of the blind spot, we concur with this theory, for the following reasons:

1. We found no form perception upon the blind spot even during the first moment of stimulation. What few inferences of form our Os obtained could quite well have been secured by noting the differentiations of brightness on the adjoining receptors.

2. When one color completely surrounded another in the blind spot our Os were never able to see the two colors dis-

tinctly. When the brightness of one varied with respect to the brightness of the other an impression of color fusion appeared at the moment of equality of brightness. These facts can easily be interpreted in terms of the stimulation of receptors outside of the blind spot.

3. We found no appreciable qualitative difference between a near liminal light on the blind spot and a near liminal light on the fovea. It is sometimes true, however, that colors seen in the blind spot are reported to be qualitatively different from those seen outside of the blind spot. This would be expected if precautions were not taken to compare a light of a near-liminal value for the blind spot with a light of near-liminal value for the fovea rather than to compare the effect of a light of given intensity as it affects both areas, because a liminal light value for the blind spot is a comparatively bright light for the sensitive retina adjacent. When a barely perceptible light on the blind spot is compared with a just-noticeable light on the retina, the reports of our Os indicate that the lights are qualitatively much alike. But even if there were some slight apparent differences, they could be accounted for by the fact that the external receptors which mediated the color perception were stimulated in a different way when they were struck by the light *after* it was reflected from the blind spot than when they were stimulated directly, on account of the different reflection, refraction, and absorption factors that were involved.

4. According to the results of our investigation given in Section E (in agreement with experimentation by Scofield and Koffka), stimuli falling on the periphery of the blind spot are seen more easily than stimuli falling on the center. This is what would be expected if the receptors around the blind spot mediated the vision, but it is difficult to explain in terms of the optic nerve theory.

5. The vagueness of dim light impressions upon the blind spot favors the integrative adjacent receptor theory. Only when the contrast between the light and the background was considerable so that the stimulus light obviously spread out over the unstimulated adjacent receptors did Os get any im-

pression of coredness. What coredness an O gets at intermediate intensities we should interpret as the resultant of the totalizing and the contrast functions of the retina. In other words, when the adjacent receptors on the retina are stimulated in a qualitatively different manner from the rest of the retina they function integratively to give a localized totalized light or core. The totalizing factor works at all intensities of stimulation, but only when the contrast factor becomes significantly great at intermediate stimulations does the resultant of these two functions produce a moderate degree of coredness.

6. The very low limen of light perception of the blind spot argues strongly against the theory of direct optic nerve reception. If nervous tissue is sensitive to light at all, it is hardly probable that it will respond to such very weak stimuli. Since the limen at the edge of the blind spot is considerably lower than at the center it would seem probable that the adjacent receptors mediate the perception.

7. The fact that the blind spot becomes adapted like the retina proper, its adaptation proceeding fairly regularly for thirty or forty minutes—seems to indicate that retinal receptors have something to do with the process.

8. The observation that light does not take on the form of the blind spot when adjacent parts of the retina are illuminated but the blind spot is kept dark, has, as we see it, little argumentative weight against the adjacent receptor theory. On account of the well recognized totalizing function of the eye *we should not* expect an absence of sensory elements to produce the presence of form. The gaps between the retinal receptors are filled in for us by a completory process and, in a like manner, the larger gaps such as those caused by the entrances of the optic nerves.

9. The reason why stimulation of the blind spot is located upon the blind spot, in spite of the fact that receptors upon the rest of the retina mediate the perception, is more difficult to explain than any of the above phenomena, but it is not at all impossible. The well known totalizing (Fuchs), shrinkage (Ferree and Rand), and formedness (Koffka) factors furnish us with the most important clues. If, as Koffka tells us,

"the field-conditions for close 'formedness' are especially favorable in the blind region," [7] then we have at our disposal a phenomenal integrative factor which is most significant. If the receptors immediately adjacent to the blind spot function integratively to produce a shrinkage and a oneness of form and quality,[8] then should we not also expect their location values to be interpreted as a oneness of location? The integrative functioning of the receptors around the blind spot seems to be analogous in many ways to the integrative functioning of cutaneous receptors, say, on the middle of the back, where two separate touch stimuli may be experienced as one even though the distance between them is as great as 6 cm. On the more frequently used extremities of our bodies the two point threshold is quite low, just as it is on the fovea of the retina; and in the same way that the two point cutaneous threshold is high on the large unused areas of the body, the two point visual threshold seems to be quite large on the sensitive regions on opposite sides of the blind spot where a nonsensitive region lies between the two points. This difference in spatial localization values, formedness and shrinkage on the receptors adjacent to the blind spot seems to give an explanation as to why we locate the light stimulus approximately on the blind spot.

[7] K. Koffka, *Brit. Jour. Psychol.*, 14, 1924, 271.

[8] See p. 18.

(Manuscript received May 11, 1931)

BILATERAL TRANSFER OF THE CONDITIONED RESPONSE IN THE HUMAN SUBJECT

BY JAMES J. GIBSON, ELEANOR G. JACK, AND GERTRUDE RAFFEL

Smith College

During the course of an experiment on the conditioned withdrawal response of the hand to an electric shock,[1] the opportunity presented itself to test for transfer of this conditioned response from the trained right hand to the untrained left hand. The subject sat at a table with the palm of the hand resting on one large brass electrode and the middle finger on another smaller one. The hand was not strapped down or constrained in any way. The apparatus and electrical circuit were essentially the same as in Watson's research,[1] except that the arrangement for graphically recording the responses on a kymograph drum was not employed. One of the Es simply sat at the side of S and recorded the finger movements as they occurred. In order to keep the attention of S off the experiment, and to lessen any anticipatory fear of the electric shock, S was required to read aloud during an experimental sitting. The conditioned stimulus was the sound of an electric buzzer lasting for one second and starting approximately $\frac{1}{10}$ sec. before the shock. This timing was accomplished by using a double contact key which, when depressed, first completed the buzzer circuit and then the shock circuit.

The procedure was as follows. After S was seated with the right hand on the electrodes, and had begun to read aloud, the buzzer was sounded alone. For several subjects who were suspicious or fearful, the sound by itself was adequate to produce a withdrawal movement. Although this response always disappeared after one or two repetitions of the buzzer stimulus, the records of such subjects have been omitted from

[1] For the original research on this subject, see J. B. Watson, The Place of the Conditioned Reflex in Psychology, *Psychol. Rev.*, 1916, 23, 89–116.

the data on transfer. After the above test, combined stimulations of buzzer and shock were given at irregular intervals varying from 3 to 20 seconds. When 10 repetitions had been completed one of two procedures was used. (1) In the first, the buzzer was sounded alone and any conditioned withdrawal movement was noted. If none appeared, another series of reinforcements was given and a second test for the conditioned response made. When the latter made its appearance, a final series of reinforcements was given and then S was told to shift over and place the *left* hand on the electrodes, with the middle finger on the far electrode as before. The buzzer was now sounded alone and any withdrawal movement noted. (2) In the second procedure, instead of waiting for the conditioned response to appear in the right hand, E made a test for the transferred response in the left hand immediately after the first 10 combined stimulations. If none appeared, S was required to shift back to the right hand and more reinforcements were given, after which another test for transfer was made. As a check on the transferred response when it occurred, a final test for the conditioned movement in the *right* hand was subsequently made.

Results.—Twenty subjects were used in the experiment. Of these, 19, or 95 percent, were successfully conditioned to withdraw the finger of the right hand when the buzzer was sounded alone. From 10 to 52 repetitions of the combined stimuli were necessary to establish the response. (The one subject who failed to become conditioned had undergone electrical treatments for a year, and showed a very weak withdrawal response even when the shock administered was much stronger than that usually employed. At the end of 200 repetitions no conditioned response had occurred and the experiment was stopped.) Of the 19, 6 showed some indication of responding to the buzzer alone before the experiment was begun, and their results have accordingly not been considered. Thirteen subjects remain for whom the data on transfer of the conditioned response seem valid. Although at no time did any subject in this group receive a shock with the left hand, 8, or 62 percent made definite withdrawal responses of

the finger when the left hand was placed on the electrodes and the buzzer alone was sounded. Three of this group had given evidence of a conditioned response of the right hand before the left hand was tested, whereas 5 showed the response in the left or transfer hand first and subsequently in the right.

It is possible that a larger number of subjects might have manifested transfer if the experiment had been continued longer. No more than 100 combined presentations of buzzer and shock were given to any one subject. If by that time no indication of transfer had shown itself the attempt was abandoned. This was done in part out of consideration for the subject for whom the experiment was naturally somewhat uncomfortable, and in part out of a desire to investigate other problems in addition to the transfer phenomenon during the experimental sitting.[2]

The appearance of the conditioned withdrawal response in the right hand itself was found to be somewhat irregular and unstable, this finding being in accord with the results of previous experiments.[3] In 2 subjects, only one definite conditioned response could be elicited, all attempts thereafter being negative. These subjects were apparently surprised and somewhat chagrined to find that they had reacted to the sound of the buzzer alone, and thereafter set themselves 'not to be fooled more than once.' They constitute two of the group for whom transfer could not be demonstrated. In general the reports of subjects indicate that various attitudes such as timidity towards shocks, a desire to appear indifferent, a desire not to be tricked into responding, and the like, played a large part in the experiment, and were the probable explanation of much of the irregularity with which the conditioned response showed itself. In view of this instability of the response in the right hand it is not surprising that the transferred response in the left hand could not always be demonstrated.

[2] These additional problems had to do with the fact of so-called 'sensory irradiation' (Pavlov) and the establishing of differential responses to buzzers of different pitch.

[3] J. B. Watson, The Place of the Conditioned Reflex in Psychology, *Psychol. Rev.*, 1916, 23, 89–116. G. Humphrey, The Effect of Sequences of Indifferent Stimuli on a Reaction of the Conditioned Reflex Type, *J. Abn. and Soc. Psychol.*, 1927, 22, 194–212.

Discussion.—In a significant number of cases (62 percent) the establishing of a conditioned withdrawal response of the right middle finger is accompanied by the formation of a similar conditioned response of the corresponding finger of the other hand. From this fact alone little can be concluded with certainty. Chiefly it suggests the need for further research on such questions as the extent of transfer to other (unsymmetrical) fingers of the same and the opposite hand, the latent periods of transferred responses as compared with the latency of the primary conditioned and unconditioned response, the possible occurrence of implicit movements in the transfer hand, and the like. But it also suggests a possible explanation towards which further experiments may be oriented.

It is clear in the first place that the conditioned withdrawal movement can scarcely be considered a single isolated reflex, or even a conditioned reflex, if by the term we mean a response limited to a specific muscle or muscle group. This latter criterion is frequently regarded as definitive for the true reflex. The hypothesis is suggested that the conditioned withdrawal to shock involves, or perhaps is itself, a generalized habit of avoiding or withdrawing when the buzzer is heard, which may be evoked from another part of the body than that in which the response was learned. Unfortunately no systematic data were obtained as to transfer of the response to other fingers. It was, however, informally determined with several subjects that the conditioned response did readily transfer to the index finger of the same hand. The extent to which a general avoidance response to the buzzer was set up can only be inferred. But from the above evidence and from what is known of transfer of learning to symmetrical and unsymmetrical parts of the body [4] it is possible to suppose that the withdrawal response could have been elicited in the other fingers and perhaps even in other parts of the body.

It should be noted that the conditioned response which is concurrently established in the untrained hand, is really a latent or potential response. It becomes actual and overt

[4] See for example C. W. Bray, Transfer of Learning, *J. Exp. Psychol.*, 1928, 11, 443–467.

only when the finger of the untrained hand is for the first time resting on the electrode. Our hypothesis would have it that in the adult, organized habits of withdrawing or pulling away have been developed for all the parts of the body which have frequently come in contact with painful stimuli. We can then suppose that in our experiment, the conditioned stimulus has become effective not only for the particular withdrawal response in connection with which it was learned, but also for this entire repertory of avoiding reactions. Furthermore when any part, say a finger, is in a potentially painful situation (*e.g.*, resting on an electrode) we may conceive that the appropriate withdrawal response for that finger is in a state of readiness—perhaps of sub-activation. It is this particular finger, therefore, which is withdrawn under such circumstances, rather than any other. Consequently the difference between the latent or potential conditioned response and the actual one depends on the presence of a specific preparatory set aroused by the situation.

On the conscious side, in so far as these activities are represented in consciousness, the above formulation would run as follows. The conditioned stimulus has become during training a general signal to withdraw. Together with this habit, there is present (normally) a definite preparatory set to withdraw the particular finger which is resting on the electrode. Hence when the finger of the untrained hand is placed in that situation, the sound of the buzzer sets off the proper response.

The introspective reports given by the subjects indicated not only that the set or attitude was an important factor in the experiment but also that the responses, both conditioned and unconditioned, could not be classed as definitely either voluntary or involuntary. They were not completely voluntary in the sense of being intended, as, *e.g.*, a finger movement in the reaction time situation is intended. The conditioned responses were usually accompanied by a feeling of surprise. On the other hand, they were not wholly involuntary in the sense of being automatic as is the knee jerk. The conditioned responses could have been inhibited had the subject wished

to inhibit them. The cases of the two subjects who set themselves not to be fooled more than once are in point. That the unconditioned response itself could be inhibited in large part was shown by one subject who took the instructions to forget about the finger on the electrode so far as possible, as meaning that he should not respond voluntarily. The finger was so successfully "forgotten" that the only response to the shock was a slight trembling or twitching movement, even when the stimulus was painfully strong. From the above evidence it appears even more clear that avoidance to shock is not a reflex. It is rather a response high enough in the scale of complexity to be in part dependent on a voluntary set.

Since a conditioned response can be formed between a stimulus and a response which was never overtly made during the conditioning process, the inadequacy of the simple diagram or physiological schema used frequently in explaining conditioning is once more demonstrated. Any real physiological explanation of the present facts would have to be based on an adequate theory of the physiological basis of bilateral transfer. Such a theory has not yet been formulated. It is possible that further research on transfer of the simple type of learning here described will contribute to this problem.

(Manuscript received May 15, 1931)

CONDITIONING THE HUMAN PLANTAR REFLEX [1]

BY WALTER C. SHIPLEY

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Experimentation on humans has revealed that not only are there gross differences in the readiness with which individuals acquire conditioned reflexes, but also within a given individual certain reactions or reflexes become conditioned much more readily than others. It seems likely that future work on conditioning in humans is going to demand that increasingly more attention be paid to the inter-relations between conditioned reflexes. Because of their idiosyncrasies, certain ones may suggest themselves as being better adapted to laboratory investigation than others. It was with a view to the possibility of extending the number of available conditioned reactions for future investigation that the present study was carried out.

Subjects

The subjects employed were ten [2] students at Yale University. These, because of the relative markedness with which they showed the normal plantar reflex, were selected from a larger group of about twenty-five.

Apparatus

The original reflex was evoked by the drawing of a small blunted brass rod rapidly along the outside edge of the sole from the heel to the ball of the foot, and then with continued rapid motion at right angles across the ball (*C*, Fig. 1). This stimulating device was so constructed [3] that the slight pressure produced by contact with the sole of the foot caused the

[1] The writer wishes to express his appreciation to Professor C. L. Hull, at whose suggestion this investigation was carried out.

[2] One subject who was run through the experiment but showed no conditioning was not included in this group because it was found that he had a few days previously been subjected to rigorous experimental extinction in another conditioning experiment.

[3] Designed and constructed by Robert R. Sears.

closing of a delicate electric switch in circuit with an electro-magnetic signal marker.

A fairly loud-toned electric buzzer in circuit with an electro-magnetic signal marker was used for the extra stimulus.

For recording the reflex movements the pneumatic recording technique, in spite of its limitations, seemed to be the best suited. Records were taken of both the toe and ankle movements. A record of the toe movements was obtained by wrapping a rubber condom (*B*, Fig. 1) around the toes in

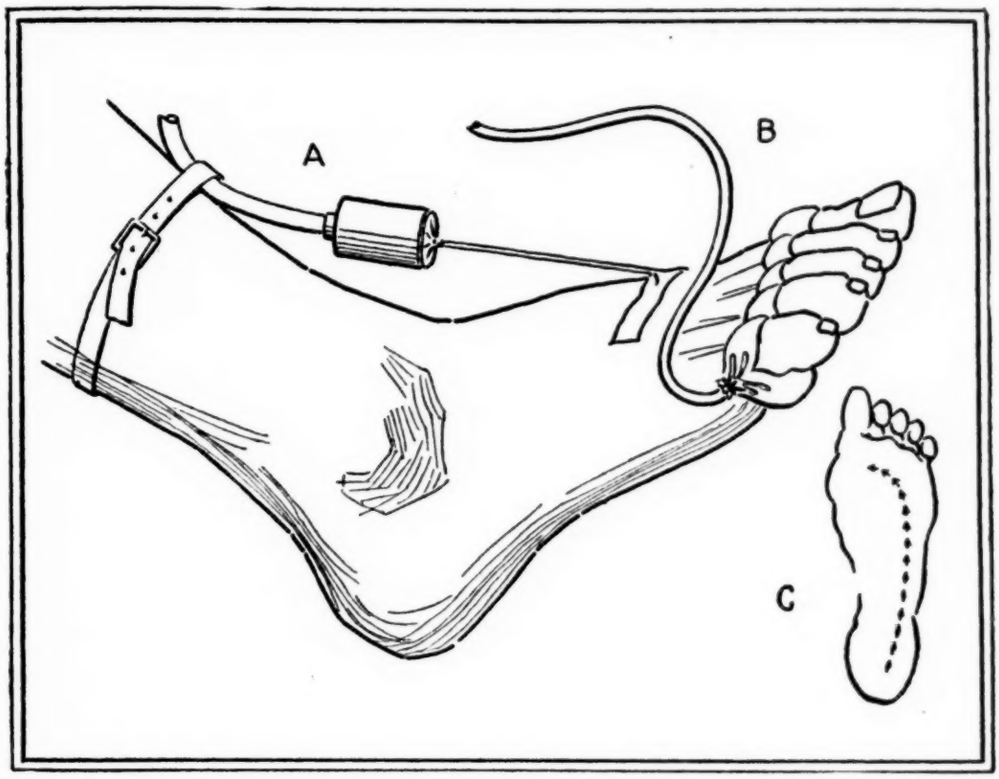

FIG. 1. Drawing showing *A*, ankle movement recorder; *B*, toe movement recorder; *C*, path of stimulation.

such a manner that when it was inflated the toes in their normal position exerted a pressure on it. Thus, in flexion the toes caused an increase, and in extension, a decrease in air pressure within it. The changes in air pressure in the condom activated a delicate recording tambour, the stylus of which was in contact with the smoked paper band on an extension kymograph. Due to individual differences in shape of the feet and toes it was found inadvisable to adhere to a rigid formula for attaching the condom to the foot, since what

was sought in each case was a record exhibiting the toe movements to their best advantage. The most generally applicable method was found to consist in encircling the toes with the condom with the bottom portion nestling in the junction of the toes and ball of the foot, and then drawing the top and bottom halves of the condom toward each other between each pair of toes by means of threads, so that the rubber fitted snugly against the bottom of each toe.

Movements of the ankle were also recorded pneumatically. A small air-tight cylinder with a rubber membrane on either end, which activated a recording tambour, was suspended by means of tapes between the ankle and foot (*A*, Fig. 1). Thus, flexion at ankle caused a fall in air pressure in the system, and extension caused a corresponding rise.

Procedure

The subject was seated with the left leg extended horizontally and the bared left foot resting on a chair directly in front of the one on which he was sitting, the foot resting in such a manner that the sole was in a vertical position and thus could be stimulated conveniently. After the above described recording devices had been attached to the foot and properly adjusted and the eyes blindfolded, a control was run to determine the neutrality of the buzzer as a stimulus. Establishment of the conditioned reaction was then attempted. In this establishment series the buzzer was sounded a fraction of a second, approximately a third, before the sole of the foot was stimulated by the passing contact of the striker. The buzzer was allowed to continue to sound until the foot stimulus had ceased. Ten to twenty combined stimulations were given, the interval between stimulations being allowed to vary between fifteen and forty-five seconds in order to eliminate the possibility of setting up a temporal conditioning. At the end of this establishment period the buzzer was sounded alone to test for the presence of conditioning. (Fig. 2 is a tracing of a portion of a typical record showing a conditioned toe and ankle movement at *X*.) If conditioning showed itself the buzzer was repeatedly sounded alone until experimental

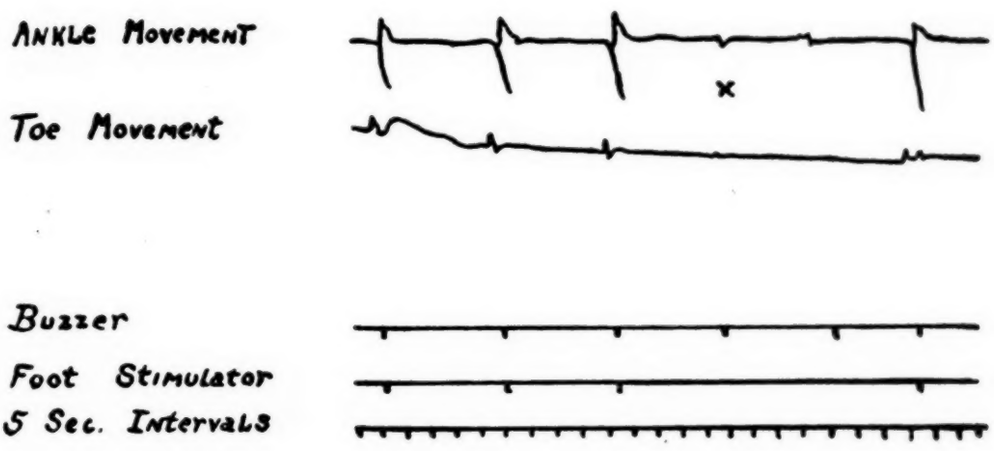

FIG. 2. Reproduction of typical record showing conditioned plantar reaction. Ankle flexion is represented by downward and toe flexion by upward movement.

extinction was complete. If no conditioning showed itself in the test, reinforcement was continued and a second test was subsequently made.

RESULTS

Of the ten subjects used in the investigation, three showed no conditioned reactions whatsoever; three showed both conditioned ankle and toe reactions, although the recorded conditioned ankle reactions were of relatively small amplitude, being about one-eighth as great as those of the reinforcement series; and seven subjects (including the three showing conditioned ankle movements) showed conditioned toe movements. Strangely enough, three of the seven exhibited what might be called 'negative' conditioned reactions, that is, where the reactions of the reinforcement series were characterized by an upward deflection on the record the conditioned reactions showed a downward deflection. This may possibly indicate that what got conditioned in these cases was some kind of defensive preparatory set. Three subjects showed evidence of conditioned toe reactions after the tenth reinforcement. For the seven subjects showing the conditioned toe reaction the records showed that the mean amplitude of the first conditioned reaction was approximately forty percent as great as the mean amplitude of the responses of the reinforcement series.

Conclusions

As a reaction to be used in investigating the mechanisms involved in conditioning the plantar reflex offers itself because of the readiness with which it becomes conditioned and because of the simplicity with which it can be recorded. Its chief limitations for this use seem to be that only about forty percent of the population possesses the reflex in a state sufficiently active to be of use in this kind of work, and that there is difficulty involved in maintaining constancy of stimulation while evoking it.

(Manuscript received May 16, 1931)

THE EFFECT OF SIMILAR AND DISSIMILAR CONDITIONS UPON LEARNING AND RELEARNING

BY JOSEPH PESSIN[1]

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There are comparatively few experiments which deal with a variation in external conditions during learning and relearning. In the field of comparative psychology Watson (11), Porter (5), Hunter (3), and Carr (1) have altered the external environment after learning occurred; and, on the whole, a decrease in relearning efficiency was found. There are even fewer experiments on human subjects, and some have been reported so briefly that it is difficult to evaluate them.

The experiment reported by Wong and Brown (12) bears indirectly upon the present study. They compared the results of work on the Yerkes multiple choice apparatus in orderly and disorderly rooms. Fewer problems were solved and the quality of the work was inferior when the subjects worked in the disordered room. The same investigators used the Trabue completion test (five forms), Whipple's symbol-digit substitution test and ink blot test, and the Woodworth-Wells direction test and symbol cancellation test. The results obtained on the latter tests in the two rooms were not significant enough to warrant publication. Pan (6) found that learning and recall were influenced by the nature of the context material appearing above and below paired associates. The presentation of words logically related to the response words aided learning and recall of paired associates more than other forms of contextual material.

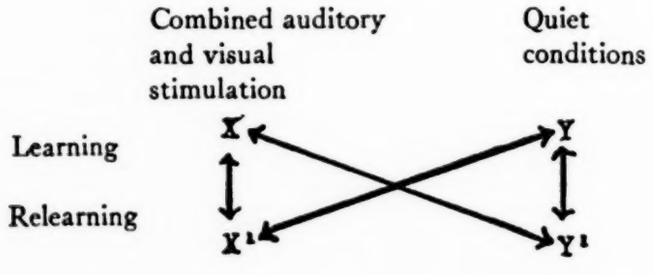
The present experiment is more closely related to those reported by Smith and Guthrie (9). In the first experiment reported by Smith and Guthrie, ten subjects learned nonsense syllables in the laboratory and out of doors. Seventy-two

[1] The writer is indebted to Professor Hulsey Cason and Mr. Ross Stagner for helpful suggestions and criticisms.

hours later some lists were relearned under similar conditions, and other lists under different conditions. It was found that a saving of 11.4 percent in terms of repetitions resulted when the material was relearned under the same conditions as it had been learned. In a second experiment lists of letters were copied on a typewriter whose keys were disarranged. An olfactory stimulus was used as the variable factor, and it was either present or absent when lists of words were typed. Twenty-four hours later the same lists were typed under similar or under different conditions. Thirteen subjects were used, and a time saving of 9.4 percent was found when the second task occurred under similar conditions. In these experiments no mention was made of the manner of presenting the nonsense material, the degree of learning efficiency, and the statistical reliability of the obtained results. The number of subjects used were also too few to yield conclusive results.

Purpose and General Plan of This Experiment.—The purpose of this experiment was to determine the effect of similar and dissimilar conditions of learning and relearning under strict laboratory control. If an individual learns material under certain laboratory conditions, will he be able to relearn this material more efficiently under similar or under dissimilar conditions? The conditions used in the present study were (*A*) combined auditory and visual stimulation or (*B*) a state of relative quiet, and the various procedures used are shown in the following diagram:

FIG. 1



X represents a learning condition in which combined visual and auditory stimuli were presented approximately every second, and *X'* refers to relearning under these same conditions. In the case of *Y* the combined auditory and visual stimuli were absent during learning, and in *Y'* these stimuli

were absent during relearning. The four procedures which were used are as follows:

(1) XX' . . . Learning and relearning with the combined auditory and visual stimuli.
(2) YY' . . . Learning and relearning under quiet conditions.
(3) XY' . . . Learning with combined auditory and visual stimulation and relearning under quiet conditions.
(4) YX' . . . Learning under quiet conditions and relearning with combined auditory and visual stimulation.

In procedures (1) and (2) the conditions of learning and relearning were similar, and in procedures (3) and (4) the conditions were dissimilar.

Material and Apparatus.—Twenty-eight nonsense syllables arranged in four lists, of seven syllables each, were pasted on an oil-cloth band which fitted over the drum of an improved model of Hull's (2) exposure apparatus. The machine was mounted on a table (39″ × 4″ × 18″), and it exposed one syllable at a time for a period of 1½ seconds. An electric buzzer was connected parallel to two electric light bulbs (100 and 50 watts), and buzzer and lights were mounted on a board (96″ × 11½″ × ¾″). The buzzer and lights were controlled by a telegraph key which was nailed on the table adjacent to the exposure machine. The current used was drawn from the commercial AC circuit, and the amount of current passing through the buzzer was regulated by a rheostat. The experimenter and the apparatus were screened from the subject, and the subject faced the windows of the exposure machine which filled an opening in the screen. The list of nonsense syllables which were used are as follows.

List (*a*)	List (*b*)	List (*c*)	List (*d*)
dod	nin	gax	rax
lez	loz	fem	mul
tuj	gaj	duj	jif
ret	kes	sar	kev
pam	tem	nuf	saf
nij	ron	kiv	tib
kuk	jik	mog	goj

SUBJECTS

Thirty-two undergraduate students acted as subjects. There were three experimental college students, thirteen sophomores, fourteen juniors, one senior, and one agriculture student. Eighteen were males and fourteen were females.

Procedure.—The experiment was conducted in a small room (12′ × 8′ × 11′), and the writer acted as experimenter in all cases. The subject was seated before the

exposure apparatus, and the board to which buzzer and lights were attached was four feet directly above his head. The subject was at first given the following typewritten instructions.

"This is not an intelligence test. We are not testing your capacity or ability. Our purpose in this investigation is to find some facts about learning. You are, therefore, requested to learn the material which will be presented on the exposure machine in front of you. You are to learn the syllables by anticipating them *A L O U D.* That is, before the syllable appears at the exposure window you are to pronounce it *OUT LOUD.* When the material is presented for the first time you will not be able to anticipate any syllables. But commencing with the second presentation you are expected to anticipate some syllables *OUT LOUD.* The material will be presented repeatedly and you are asked to anticipate the syllables out loud until the experimenter closes the exposure window. Please do not read the syllable which is exposed, anticipate the syllable before it appears. If you do not understand the instructions, please read them again."

During the period which preceded the actual experimentation, the subject learned and relearned one practise list under conditions XX', and another practise list under conditions YY'. The results obtained during the practise period were discarded, and these nonsense syllables were not used in the main experiment. The lists of nonsense syllables were rotated in such a way that each list was learned in all temporal positions by different subjects. The learning program which was used to control the learning conditions is shown in Table I.

TABLE I

LEARNING PROGRAM SHOWING THE ROTATION OF THE VARIOUS CONDITIONS

Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	Group 8
XX′	YY′	XY′	YX′	XX′	XY′	YY′	YX′
YY′	XY′	YX′	XX′	XY′	YY′	YX′	XX′
XY′	YX′	XX′	YY′	YY′	YX′	XX′	XY′
YX′	XX′	YY′	XY′	YX′	XX′	XY′	YY′

The specific procedure with each individual may be illustrated by using the first subject as an example. Subject No. 1 learned list *a* under condition X to the point of one perfect reproduction. He was then given a copy of Pieron's book, "Thought and the Brain," and was instructed orally as follows: "Turn to any page in this book and read out loud until I tell you to stop. Do not try to get any meaning out of the material you read, but just read the words." After four minutes of reading, subject No. 1 relearned list *a* under condition

X'. List *b* was learned and relearned in a similar manner under conditions YY', list *c* under conditions XY', and list *d* under conditions YX'. A four-minute rest was allowed before taking up each list. There were four subjects in each of the groups designated in Table I.

Results.—The two measures of learning used were (1) the number of errors made, and (2) the number of repetitions required to reach the point of one perfect reproduction. The Ebbinghaus saving method was used as a basis for comparing

TABLE II

THE PER CENT SAVED DURING RELEARNING UNDER SIMILAR AND UNDER DISSIMILAR CONDITIONS

No. of the Subject	Group	Condition XX'		Condition YY'		Condition XY'		Condition YX'	
		Errors	Repet.	Errors	Repet.	Errors	Repet.	Errors	Repet.
1	1	63.16	50.00	89.13	86.67	74.29	70.00	41.67	18.18
2	2	85.71	80.00	70.59	66.67	91.30	97.14	64.29	50.00
3	3	−37.50	00.00	56.25	60.00	60.71	62.50	33.33	50.00
4	4	−53.85	−100.00	79.17	57.14	75.68	72.73	57.69	42.86
5	5	63.64	50.00	58.06	50.00	58.06	42.86	69.52	75.00
6	6	84.72	83.33	82.50	77.78	75.00	71.43	76.92	55.56
7	7	65.71	40.00	75.00	66.67	71.43	40.00	88.23	77.78
8	8	80.65	76.47	59.26	28.57	74.55	71.43	84.61	75.00
9	1	69.56	71.43	48.28	37.50	90.91	77.78	100.00	87.50
10	2	81.39	73.33	76.47	54.55	32.00	14.29	44.44	20.00
11	3	76.92	72.73	80.49	75.00	40.00	37.50	81.08	77.78
12	4	75.00	62.50	77.42	55.56	82.46	60.00	96.77	77.78
13	5	71.43	50.00	58.82	50.00	72.73	55.56	85.18	57.14
14	6	60.87	42.86	89.29	77.78	80.77	71.43	81.08	71.43
15	7	74.29	58.33	80.00	63.64	69.44	69.23	73.68	66.67
16	8	− 5.56	00.00	60.00	63.64	75.00	77.78	39.39	25.00
17	1	97.73	81.82	76.32	66.67	82.05	66.67	75.00	64.29
18	2	63.64	57.14	80.00	78.57	79.41	55.56	81.48	62.50
19	3	94.44	83.33	33.33	42.86	73.08	77.78	86.11	81.82
20	4	90.00	77.78	69.56	33.33	82.14	60.00	78.57	55.56
21	5	50.00	50.00	77.27	50.00	72.97	54.55	66.67	57.14
22	6	91.67	81.82	70.27	44.44	89.39	70.59	95.83	86.67
23	7	82.81	72.73	66.67	57.14	89.80	80.00	65.62	57.14
24	8	91.84	85.71	73.91	33.33	90.32	66.67	76.47	66.67
25	1	68.75	57.14	86.16	62.50	89.66	77.78	100.00	88.89
26	2	58.62	42.86	73.68	66.67	40.00	44.45	75.00	66.67
27	3	82.61	75.00	87.50	77.78	96.30	75.00	70.00	50.00
28	4	91.89	76.93	71.87	70.00	37.93	25.00	90.91	81.82
29	5	60.00	50.00	40.00	37.50	85.71	77.78	71.87	63.64
30	6	72.00	81.82	83.72	86.67	40.00	50.00	78.38	72.73
31	7	66.67	57.14	66.67	71.43	86.36	77.78	41.18	42.86
32	8	35.29	33.33	58.82	60.00	50.00	44.44	12.50	16.67
Means		64.19	55.49	70.51	59.69	72.17	62.37	71.36	60.71
S.D. (Dis.)		33.45	34.28	13.56	15.86	17.38	17.63	21.06	19.48
P.E. (Mean)		3.99	4.09	1.62	1.89	2.07	2.10	2.50	2.32

the conditions investigated in this experiment. The results of the whole experiment are shown in Table II.

Table II shows the average percent saved for each of the four conditions, the standard deviation of the distributions (S.D.$_{(dis.)}$), and the probable errors of the means (P.E.$_{(mean)}$). The greatest average percent saved both in terms of errors and repetitions occurred under conditions XY'. The next best situation from point of view of percent saved during relearning was YX'. The poorest condition was XX', and next to poorest was condition YY'. These results indicate that the similar conditions XX' and YY' as they were devised in this experiment are slightly less advantageous to the learning process than the dissimilar conditions XY' and YX'. The differences, however, are small and not large enough to insure complete reliability as is shown by the critical ratios in Tables III and IV.

TABLE III

DIFFERENCES IN THE AVERAGE PER CENT SAVED IN TERMS OF ERRORS UNDER DIFFERENT CONDITIONS, AND THE CRITICAL RATIOS

Conditions		Difference in % Saved	P.E. of Difference	Critical Ratio Diff./P.E.diff.	Chances in 100 of a True Diff. Greater than Zero
1st	2d				
XY′	YX′	.81	3.25	.25	57
XY′	XX′	7.98	4.49	1.78	89
XY′	YY′	1.66	2.63	.63	67
YX′	XX′	7.17	4.71	1.52	84
YX′	YY′	.85	2.98	.29	58
YY′	XX′	6.32	4.31	1.47	84

In Table III all conditions represented in the first column have a slightly greater percent saving during relearning than the conditions represented in the second column. The differences between the conditions listed in the first column and those appearing in the second are shown in column three. The P.E. of the difference and the critical ratios are also shown. Since a critical ratio of four is accepted as indicative of complete reliability, the critical ratios in Table III are, therefore, not large enough to insure complete reliability. The next table shows the critical ratios in terms of our second

measure of learning, namely, the number of repetitions required to reach the point of one perfect reproduction.

TABLE IV

DIFFERENCE IN THE AVERAGE PER CENT SAVED IN TERMS OF REPETITIONS UNDER DIFFERENT CONDITIONS, AND THE CRITICAL RATIOS

Conditions		Difference in % Saved	P.E. of Difference	Critical Ratio Diff./P.E.diff.	Chances in 100 of a True Diff. Greater than Zero
1st	2d				
XY′	YX′	1.66	3.13	.53	64
XY′	XX′	6.88	4.60	1.50	84
XY′	YY′	2.68	2.83	.95	74
YX′	XX′	5.22	4.70	1.11	77
YX′	YY′	1.02	2.99	.34	59
YY′	XX′	4.20	4.51	.93	74

In Table IV we find that none of the differences are large enough to insure complete reliability. The differences in terms of repetitions are slightly smaller and less reliable than those in terms of errors.

Discussion of Results.—The results obtained in this experiment show that individuals are able to relearn material under dissimilar conditions at least as efficiently as under similar conditions. This may be due to the ability of human beings to adapt themselves to a change in the environmental situations. Thus in our experiment when the combined visual and auditory stimuli were presented, the subjects increased their efforts to master the material. This increased effort was manifested in the subjects' overt behavior and in some voluntary verbal reports. The subjects talked louder, made more overt movements, and expended more energy while the combined auditory and visual stimulation was present. The observations of the overt behavior seem to verify Morgan's (4) findings that human beings exert more energy and effort to overcome distracting influences in the environment. It appears that when individuals maintain a relatively high degree of effort, learning and relearning are just as efficient under dissimilar conditions as under similar conditions. This is borne out by the experimental evidence presented in this paper. Further indirect evidence is found in studies of the

effects of external conditions upon human activity.[2] The implications from the fact that the dissimilar conditions are slightly superior are not quite clear. We may, however, hazard the suggestion that few people work at their maximum level of efficiency in an experimental situation where they are motivated by many factors not present in daily life. When human beings are challenged by a change in the conditions, they may very well increase their efficiency level, exert more energy and as a result show actually superior performance. That motivation is a major factor involved is at least a highly probable conclusion.

Summary and Conclusions.—The relative efficiency, as measured by the Ebbinghaus saving method, of similar and dissimilar learning and relearning conditions was studied. The conditions used were: (A) a combined auditory and visual stimulation or (B) a state of relative quiet. No significant differences were found in the percent saved during relearning under the various conditions devised in this experiment.

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(Manuscript received May 28, 1931)

PERIODICITY OF RECOVERY DURING THE REFRACTORY PHASE OF THE EYELID REFLEX [1,2]

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'Absolute' refractory phase may be defined as a period of diminished excitability following primary stimulation of a neural system during which no second stimulus, however intense, will excite a response. This is followed by a longer time interval during which stimuli stronger than normal are required for the same response, or, during which an abnormally small response is evoked by the given stimulus ('relative' refractory phase). Verworn demonstrated that the magnitude of neuromuscular disturbance increases progressively as the time interval between stimuli within the refractory phase is increased, *i.e.*, that the effect of the second stimulus is the less, the more quickly it follows the first. The latent period of reaction is supposed to be longer, the more quickly the first stimulus is followed by the second.

The termination of the relative refractory phase is not a simple or immediate return to normal excitability since there is commonly a supernormal phase subsequent to relative refractory phase. In nerve there has been experimentally shown to be a real increase in excitability as measured by nerve conduction and electrical response. In complex neuromuscular systems the supersensitive phase and the course of recovery is more problematical. The latter were the questions at issue in the investigation here reported.

Because of the availability of recording techniques the eyelid reflex was studied. Previous investigators of this reflex include Exner (7), Mayhew (10), Garten (8), Weiss

[1] The writer is indebted to Professor Raymond Dodge and Professor L. T. Spencer for guidance and criticism during this investigation.

[2] This paper is part of a dissertation submitted to the Faculty of the Graduate School of Yale University in candidacy for the degree of Doctor of Philosophy, 1929.

(14), Zwaademaker and Lans (15), Sherrington (12), Dodge (2), Dodge and Benedict (5), Partridge (11), and Cason (1). The first study of the refractory phase of this reflex is that of Zwaademaker and Lans, who found that, using flashes of light as stimuli, successive lid closures were impossible (due to absolute refractory phase) when the stimuli were from 250 to 500 sigma apart; that they occurred in 34 percent of the cases when the stimuli were 500 to 750 sigma apart; and that they occurred in 67 percent of the cases when the stimuli were 750 sigma to 1 second apart. The authors, therefore, consider 500 sigma to 1 second as the 'critical interval.' With air stimuli the absolute refactory phase was observed to be shorter (below 250 sigma), warmer air causing the 'critical interval' to rise.

Sherrington states that "in the eyelid reflex for nearly a full second after initiation of a reflex, the chance that a second stimulus then delivered will, though otherwise appropriate, excite the reflex, is 50 percent less than it is 1 second later. The refractory phase is marked though not absolute; it operates longer for a visual stimulus than for a tactual or thermal."

One of the most accurate techniques for recording the characteristics of the eyelid reflex is that of Dodge. For a single subject, using sudden noise stimuli, Dodge's results as to the refractory phase of the reflex differ markedly from those of Zwaademaker and Lans. As we have just said the latter authors found an absolute refractory phase of about 500 sigma for light stimuli and about 250 sigma for air stimuli. Dodge states, "My records, on the contrary, show no absolute refractory phase. In all my subjects a second wink was elicited by a stimulus that both the subjective apprizement and the records show to have been no more intense than the first within 300 sigma of the initial stimulus. In all cases within 2 seconds, however, the second reaction was lower than the first. Even when . . . the second stimulus occurs before the lid has reached its original abscissa after an interval of less than 160 sigma, the refractory phase is not absolute. To be sure the reaction consists only

in a slight hesitation of the descending limb of the curve (opening of the lid). . . . By a comparison with the normal returns . . . the reaction is unmistakable, and its apparent latent time is only slightly longer than that of the primary reaction."

Dodge states that the relative refractory phase of the reflex may last 2 or 3 seconds.

None of the previous investigations have raised the question whether or not, during the relative refractory phase, there is a uniform return of excitability from the end of absolute refractory phase to the beginning of the supernormal phase. That this question may be of some scientific importance is indicated by the data to be presented.

Apparatus

The apparatus for the present investigation included adequate means for eliciting and photographically recording both the noise stimuli and the eyelid responses to them. The 'glorified mousetrap' noise-stimulus apparatus (Dodge 7, 9, 10) consisted essentially of two steel-wire loops which act as percussion hammers when released against a sounding box. The intensity of the two noises was adjustable by varying the tension of the activating springs. In our experiment both noises were equated for quality and intensity.[3] Before stimulation the wire percussion hammers were held horizontal by

Dodge's artificial eyelash shadow method had two disadvantages. It is uneconomical of photographic paper, and gives excursions in the records which are difficult to align with lid movement. The present method (suggested by him) reflects a beam of light from a mirror at the axis of an 11″ light wooden lever, the end of which was attached to the left lid by a small piece of surgeon's tape. The distance from the recording mirror to the camera was 9″. Although this method actually increased the muscle load, all subjects reported that after a few preliminary voluntary winks they could perceive nothing attached to their lids. The total weight of the lever was 0.4 gms. The mechanical advantage gained by its leverage made the slightest movement effective in displacing the recording beam. Several records could, moreover, be taken on one sheet of sensitized paper by a slight alteration of the relations of the photographic system and the source of light.

The moment of stimulation and the lid response were shown on the same record line by a new device. A wooden projection from the sounding box touched the base on which the fulcrum of the lid mirror-recorder rested so that the impact of each hammer would simultaneously displace the mirror causing a 'break' in the continuous photographic record. The vibrations set up by the impact of the hammers were damped by placing the sounding box in a sand bed. The effectiveness of this device was such that latency readings in sigma could be made with entire confidence.

The photographic recording camera was a modification of the Dodge-Cline (12)

[3] This was done by the Method of Constant Stimuli using 5 subjects.

suitable electromagnets, the noise being produced by breaking the circuit through these electromagnets.

recording camera. Within a horizontal wooden box, a frame, fitted to carry commercial 5 by 7 inch photographic plate holders, could slide horizontally in grooves of heavy brass supports. The required slow and quiet movement of the frame and plate holder was produced by a spring phonograph motor driving a pulley arrangement which in turn operated a friction drive formed by the axle of the pulley in contact with a horizontal iron rod attached to the plateholder frame. This horizontal rod was also utilized to serve as a 'contact breaker' of the circuits through the electromagnets controlling the sound hammers. Two contacts, each in series with one electromagnet, were set up on a horizontal support in such a way that the movement of the rod would break the circuit at predetermined intervals either by varying the distance between contacts, or by varying the various pulley systems.

Timing of the records was effected by placing a Becker marker in series with an oscillating reed outside the laboratory room. This oscillator operated by alternate making and breaking of electric current 10 times per second, the Becker marker, therefore, making corresponding oscillations. A mirror similar to the stimulus-response recording mirror, placed on the tip of the marker armature, received its light from the same source as the lid recording lever, and transmitted its oscillating beam through the same cylindrical lens which controlled the light beams as to width and intensity.

A constant pre-experimental position of the lid was achieved by securing the subject's head fairly well with a mouth rest and by instructions to fixate attentively a small red cross. Respiration was not controlled, but it can be assumed that any differences due to this factor would be equalized for all subjects by the large number of trials taken.

Following Dodge's method of procedure the following events preceded each record: (1) the opaque slide of the plate-holder was withdrawn; (2) the contacts through the electromagnets were closed and the hammers set; (3) the recording mirrors were adjusted to the desired position; (4) the signal "ready" (*i.e.* to attend to the fixation mark) was given; (5) after a varying time interval the plate-holder mechanism was quietly set in motion; (6) at fixed points in its course the moving plate-holder broke the contacts which controlled the stimulus noises.

Procedure and Results

Three subjects were given the following series of stimulus-intervals each day for ten consecutive days: 0, 20 sigma, 40 sigma, 80 sigma, 160 sigma, 320 sigma, 640 sigma, 1 second, 2 seconds, 3 seconds, 4 seconds, 5 seconds, 6 seconds. Trials were separated from each other by a time interval of 30 seconds. The stimulus-intervals were presented in random order each day. Especial care was taken to fasten the lever recorder to the lid in the same position at each experimental session.

Tables 1, 2, and 3 present the readings of the amplitudes of R_1 and R_2 (reactions to the first and second sound stimuli) for each of the stimulus-intervals for each subject. The mean, mean variations, and the percentage amplitude of R_2 (of their respective average R_1's) are also presented.

TABLE 1

AMPLITUDES OF R (IN MM.) FOR SUBJECT A AT ALL STIMULUS-INTERVALS DAILY FOR 10 DAYS

Day	Zero		20 sigma		40 σ		80 σ		160 σ		320 σ		640 σ		1 sec.		2 sec.		3 sec.		4 sec.		5 sec.		6 sec.	
	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2
1	14.7		15.1		14.1	.1	17.1	.1	14.2	.1	10.2	.1	13.1	.1	18.5	9.2	17.1	11.1	15.9	9.2	13.5	10.2	5.1	6.1	5.0	4.9
2	16.1		12.2		—		—		—		—		18.2	7.2	16.5	13.2	16.0	12.5	18.1	11.2	14.5	14.0	12.2	14.5	12.6	12.1
3	21.5		18.1		17.6		20.0		19.2	.1	19.1	.1	12.0	12.1	21.5	20.1	20.6	19.8	21.3	18.1	18.1	17.5	—	—	20.5	19.1
4	—		14.9		—		9.3		10.1		13.2	.1	19.7	18.9	15.1	12.3	15.1	7.5	21.0	19.5	21.3	17.4	14.0	12.5	12.1	11.3
5	18.1		12.1		12.3		11.1		—		15.3	11.1	—	—	13.8	12.1	13.7	13.1	15.0	11.1	12.5	—	10.5	4.5	10.5	10.1
6	17.1		24.1		21.9		—		—		11.9	12.1	21.9	21.8	22.1	25.0	19.8	5.1	19.1	17.0	15.0	19.0	16.1	10.2	13.0	15.1
7	17.9		19.6		15.1	.1	25.9	.1	22.3	15.3	23.0	21.1	19.9	18.2	16.9	13.5	13.0	12.5	20.1	2.3	22.7	20.8	22.5	18.1	20.7	11.6
8	10.3		11.3		9.3		10.5		—		10.3	5.6	10.1	8.3	13.1	8.7	14.9	3.1	12.5	2.1	16.3	10.9	12.2	10.3	14.1	8.2
9	26.1		27.1		—		25.1		23.1		18.1	.1	23.1	—	24.8	19.1	19.1	7.1	18.1	—	26.1	22.2	27.5	11.3	—	—
10	24.1		—		21.9		14.5		16.6		—	.2	19.9	18.3	22.1	20.0	20.1	3.2	19.6	16.3	20.1	—	17.5	3.9	19.1	16.1
Mean	18.4		17.2		16.0		16.7		17.6		15.1	5.6	18.0	13.0	18.4	15.3	16.9	9.5	18.0	11.9	18.0	16.5	13.8	9.1	12.8	10.3
M.V.	3.6		5.0		3.8		5.3		3.9		3.7	6.1	3.7	6.2	3.4	4.6	2.4	4.2	2.4	4.7	3.7	4.2	4.9	3.9	3.8	3.6
$\% \frac{R_2}{R_1}$											37.1		72.2		83.1		56.2		66.1		91.7		65.9		80.5	

TABLE 2

AMPLITUDES OF R (IN MM.) FOR SUBJECT B AT ALL STIMULUS-INTERVALS DAILY FOR 10 DAYS

Day	Zero		20 sigma		40 σ		80 σ		160 σ		320 σ		640 σ		1 sec.		2 sec.		3 sec.		4 sec.		5 sec.		6 sec.	
	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2
1	15.9		17.2		12.9		18.0		19.6	.1	16.5	4.8	17.4	9.2	14.1	10.1	—	—	21.3	3.9	10.9	13.1	20.1	15.6	16.7	12.9
2	—		11.9		—		—		21.4		17.2	8.5	16.9	14.1	16.4	8.6	12.9	3.7	17.2	12.1	20.2	19.6	17.3	14.3	—	—
3	17.5		16.9		13.9	.1	16.7	.1	18.3		14.9	.1	14.1	8.6	—	—	17.1	16.1	18.6	9.6	16.7	14.1	14.9	12.6	15.3	15.0
4	16.2		14.7		22.1		15.0		—		16.1	9.7	—	—	18.4	6.2	10.3	.1	14.9	13.1	14.8	7.5	19.2	8.2	18.9	6.5
5	12.9		—		17.2	.1	14.8		18.5	.1	20.2	16.5	13.6	.1	13.8	14.9	21.7	14.2	15.1	6.5	13.6	8.6	20.0	—	16.9	17.1
6	15.7		10.5		18.6		19.1	.1	12.9		14.9	.7	18.8	4.1	12.9	10.5	18.6	9.8	12.7	4.9	14.9	3.1	16.6	10.5	16.8	13.5
7	15.8		14.9		17.6		—		14.7		—	.1	19.6	9.7	17.1	15.3	—	—	17.2	6.0	18.3	20.5	12.3	16.5	14.1	12.1
8	16.1		14.6		18.1		19.6	.1	15.3	.1	16.1	3.1	14.5	3.1	16.7	13.6	17.2	3.6	11.5	8.7	—	17.5	14.9	14.8	15.0	9.7
9	17.1		18.1		20.2		16.1		18.7		13.5	2.1	16.1	12.2	12.9	10.2	16.1	10.1	10.9	11.0	16.4	10.1	16.6	9.2	—	—
10	15.5		16.7		18.3		12.9	.1	17.6	.1	10.2	.1	12.9	2.6	17.5	12.7	16.3	8.1	18.3	16.5	12.7	—	18.9	7.5	16.9	8.5
Mean	15.9		15.1		17.6		16.5		17.4		13.9	4.6	16.0	7.1	15.6	11.4	16.3	8.2	15.8	9.2	15.4	12.7	17.1	12.2	16.3	11.9
M.V.	.8		1.9		2.0		1.8		2.1		2.5	4.3	2.0	4.1	1.9	2.5	2.4	4.3	2.9	3.2	2.2	4.8	2.0	2.9	1.2	2.9
$\% \frac{R_2}{R_1}$											33.1		44.3		73.1		50.3		58.2		82.4		71.3		73.0	

TABLE 3

AMPLITUDES OF R (IN MM.) FOR SUBJECT C AT ALL STIMULUS-INTERVALS DAILY FOR 10 DAYS

Day	Zero		20 σ		40 σ		80 σ		160 σ		320 σ		640 σ		1 sec.		2 sec.		3 sec.		4 sec.		5 sec.		6 sec.	
	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2
1	19.7		15.4		19.1		18.7		21.6		13.7	1.1	18.4	3.1	14.9	8.4	19.2	6.5	20.1	17.2	15.2	9.8	18.1	3.6	13.2	18.7
2	16.5		16.2		—		22.3		18.7	.1	12.1	3.1	23.1	12.5	23.7	13.7	10.7	9.3	16.2	14.1	18.7	7.5	14.3	9.5	17.4	12.5
3	15.1		16.9		16.4	.1	14.7	.1	17.1		19.7	—	15.4	3.7	9.2	6.6	18.6	5.1	11.5	11.9	19.3	—	19.7	7.2	12.9	9.6
4	17.5		13.7		17.1		16.1		15.7	.1	23.4	3.4	17.7	8.9	18.2	12.5	9.4	2.4	22.1	—	14.1	19.1	12.5	8.6	19.7	16.1
5	13.1		—		14.3		14.9		—		17.1	8.6	10.1	9.1	14.3	11.2	—	9.3	22.1	11.6	17.6	—	17.7	17.5	—	—
6	10.2		18.1		12.5		14.5		18.4		19.8	7.5	9.7	4.3	16.6	8.5	17.2	7.8	17.5	6.4	16.5	18.7	12.9	16.9	18.6	4.5
7	12.9		—		16.1		17.0		—		—	—	13.2	12.5	14.9	3.2	15.8	2.1	19.7	20.1	16.9	9.6	16.3	3.7	16.9	17.0
8	17.2		22.1		19.4	.1	14.7		16.5	.1	16.5	.1	17.3	9.3	16.7	12.5	14.1	9.6	15.2	3.5	—	12.5	15.3	3.9	12.1	20.1
9	18.6		14.9		11.1	.1	18.9	.1	21.6	.1	17.7	15.2	—	—	15.1	14.9	—	—	14.0	9.3	14.9	23.1	10.4	7.4	15.0	9.1
10	14.1		19.1		11.9		17.7		13.6		20.1	5.6	—	4.8	16.4	7.8	21.7	10.5	13.1	4.6	19.2	14.1	16.2	12.5	11.1	5.7
Mean	15.5		17.0		15.3		16.9		17.9		17.9	5.6	15.6	7.6	16.0	9.9	15.8	6.9	17.1	11.0	16.9	14.3	15.3	9.1	15.2	12.6
M.V.	2.4		2.0		2.5		1.9		2.2		2.6	3.7	3.5	3.1	2.3	3.0	3.3	2.6	3.1	4.5	1.6	4.5	2.3	4.0	2.6	4.8
$\% \frac{R_2}{R_1}$											31.3		48.7		61.8		43.6		64.3		84.6		59.4		82.9	

Table 4 presents the average means in summary form. From these data it may be noted that:

(1) R_2's were not graphically available at the stimulus-intervals lower than 40 sigma and were not available for purposes of statistical treatment at stimulus-intervals shorter than 320 sigma because of their small number.

TABLE 4

INDICES OF EXCITABILITY $\left(\frac{R_2}{R_1}\right)$ DURING REFRACTORY PHASE

Interval	Subject			Mean
	A	*B*	*C*	
320 sigma	37.1	33.1	31.3	33.8
640 sigma	72.2	44.3	48.7	55.1
1 second	83.1	73.1	61.8	72.7
2 seconds	56.2	50.3	43.6	50.0
3 seconds	66.1	58.2	64.3	62.9
4 seconds	91.7	82.4	84.6	86.2
5 seconds	65.9	71.3	59.4	65.5
6 seconds	80.5	73.0	82.9	78.8

(2) An expected decrease in amplitudes of R_1 and R_2 in the course of the 10 day interval (negative adaptation) is not found. This renders it permissible to average the values as well as to calculate mean variations. The means of all the R_1's for all subjects are within a range of 12.8 mm. to 18.4 mm., the average mean for all subjects being 16.3 mm.

(3) The M.V.'s for R_1 are less than for R_2. The mean M.V. for R_1 is 2.7 mm., and for R_2, 4.0 mm., half again as great as that of R_1. This indicates a variability of the amplitude of response within and after the refractory phase considerably greater than that of the response following primary stimulation.

(4) R_2, where recorded, is less than R_1 for all intervals. According to definition, all these R_2's would be considered in relative refractory phase. Data presented in Table 4, calculated from the R_2/R_1 ratio indicate the excitability of the reaction-system at the moment when R_2 was evoked as compared with the excitability of the reaction-system at the moment when R_1 was evoked. These figures, of

course, represent the relative amount of R_2 as compared with its respective R_1.

(5) Instead of the expected progressive increase in amplitude indicating increased excitability of R_2 as longer intervals within the relative refractory phase are measured, it may be seen that, although for each individual and for the group this progressive increase does occur through 1 second, there is a reversion of excitability at 2 seconds initiating another progressive increase through 4 seconds. A reversion again occurs at 5 seconds, followed by an increased excitability at 6 seconds. In no case do these reversions fall to levels of excitability as low as previous reversions. The relative refractory phase, therefore, apparently can be considered as a phenomenon of diminished excitability which becomes restored, not in simple arithmetical progression, but rather in a rhythmical manner which follows a wave-like form. This is presented graphically in Fig. 1.

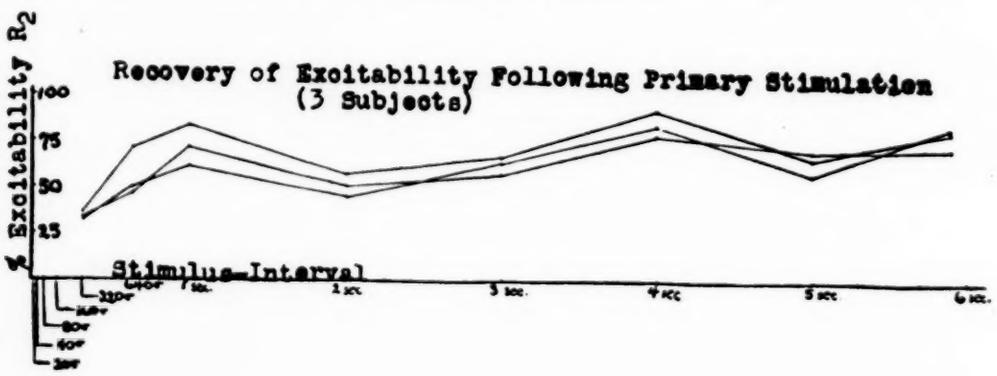

FIG. 1

(6) The supernormal phase cannot be definitely determined from these data if one must measure it from average R_2 amplitudes of greater magnitude than their respective R_1 amplitudes. R_2's greater than R_1's were occasionally noted at the short stimulus-intervals, and occur progressively more frequently as the stimulus-intervals become longer. Supernormal phase probably occurs no sooner than the 3 second stimulus-interval, and may last longer than 6 seconds; there is presumptive evidence, at least, that there is a rebound for this reflex which lasts as long as, if not longer than, the refractory phase, viz., about 3 seconds.

(7) There is marked individual variability in respect to amplitude of lid movement and duration of the refractory phase. The wave like-form of recovery of excitability within the refractory phase, however, presents points of flexion which are analogous for all subjects. Variability at the several points of the curve is relatively small. It is probable, therefore, that despite individual variability there is a consistent wave-like form in the restoration of excitability following primary stimulation.

(8) The latent period was of the order of 40 sigma, within a range of the order of 23 sigma to 49 sigma. These figures confirm Dodge's findings (10). The latencies of the R_2's indicated a mean figure and range of no markedly different magnitude from that of R_1.

(9) The absolute refractory phase of the reflex, if it exists, was seen to be less than 40 sigma; Dodge's failure to find any absolute refractory phase for the wink is, therefore, confirmed since responses to secondary stimulation at intervals within the latent period of primary response cannot be considered suitable conditions for determining refractory phase.

(10) Consistent rebound, or supernormal phase, could not be found for this reflex. One would be forced to rely on the statistical average of amplitudes of response to determine this. The long intervals measured, including the range from 3 seconds to 6 seconds, were shown to contain an increasing number of cases in which the second responses were definitely greater than their respective first responses. Average second responses of greater magnitude, however, were not obtained. If the occurrence of these increased second responses is an indication of supernormal phase, it must be presumed that the supernormal phase lasts at least as long as the relative refractory phase, with the probability remaining that its duration may be even longer. Within this period also a rhythmicity of excitability is demonstrated.

Conclusion

The recovery for the lid reflex does not develop in simple arithmetical progression, but in well-defined waves. The

similarity of these waves for three subjects, as shown by our records, makes it highly probable that there is a generally consistent rhythmicity of excitability within the refractory and supernormal phases. Since the fluctuation of excitability within relative refractory phase occurs on the average below the level of normal excitability this condition of alternating recovery and depression might be termed 'relative recovery from refractory phase.' The genesis of rhythmic response may depend on this fundamental rhythmicity of recovery from refractory phase.

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(Manuscript received June 23, 1931)

NEGATIVE ADAPTATION AND REFRACTORY PHASE IN THE EYELID REFLEX [1, 2]

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The diminution and eventual disappearance of a reflex response consequent to repeated stimulation (negative adaptation) has been studied by Griffith (7), Dodge (4) (5), Cason (2), Dodge and Louttit (6) and the writer (3). Griffith (7) and Dodge (4) have shown that this phenomenon exists in post-rotation nystagmus and compensatory eye-movements, and Dodge has demonstrated a "typical" curve which drops rapidly at first with subsequent negative acceleration. Negative adaptation of the eyelid reflex has been demonstrated by Cason (2), and by Dodge (5). In the knee-jerk, however, Dodge has shown that diminished amplitude of response to repeated stimulation is extremely small over a period of some 20 months.

Dodge and Louttit (6), in investigating the body-start reflex and the ear reflex of the guinea pig to sudden noise, demonstrated that the speed of negative adaptation to repeated stimulation was greater for the former reflex than for the latter. They attributed this difference to the fact that the relative refractory phase of the body-start reflex is longer than that of the ear reflex. With this experiment as a basis, the writer (3) studied variation in the rate of negative adaptation of the body-start reflex of the guinea pig when stimulation occurred at different intervals in the refractory phase. It was shown in that investigation that there is a relationship between the speed of negative adaptation and various intervals within the refractory phase of the reflex at which stimulation falls, *i.e.*, there were points of greater and smaller

[1] Grateful appreciation is due to Professor Raymond Dodge and Professor L. T. Spencer for frequent advice and criticism during the course of this investigation.

[2] This paper is part of a dissertation submitted to the Faculty of the Graduate School of Yale University in candidacy for the degree of Doctor of Philosophy, 1929.

susceptibility to negative adaptation which followed the form of a modal curve.

The present investigation is an extension of the inquiry into the relationship between negative adaptation and excitability of response at various moments in the refractory phase. The eyelid reflex was studied because information regarding the recovery of excitability during its refractory phase was at hand (2), and because of available recording techniques.

Apparatus

The apparatus consisted essentially of a camera which recorded the movements of a light beam, reflected from a mirror on a lever attached to the eyelid. Noise stimuli were provided by wire hammers released electromagnetically at appropriate moments against a sounding box.[3]

Procedure and Results

Fifty-five adult subjects, of whom 26 were women, were apportioned by chance to 11 stimulus-interval groups of 5 subjects each. The stimulus-intervals selected were 0, 20 sigma, 40 sigma, 80 sigma, 160 sigma, 320 sigma, 640 sigma, 1 second, 2 seconds, 3 seconds, and 4 seconds. Sixty trials, spaced at 30 second intervals, were given for each subject. The subjects were unaware of the number of trials to be recorded. Except for the slight sounds made in adjusting the apparatus, and occasional introspective reports, the laboratory was relatively silent, except, of course, at moments of stimulation.

Table 1 presents average amplitudes of R_1 and R_2, responses to the two stimuli at the given interval, of five consecutive trials for each group throughout the series.

(1) It may be seen that data of R_2 were not available for purposes of statistical treatment until the 320 sigma interval. Although occasional responses to the second stimulus were evoked at the 40 sigma, 80 sigma, and 160 sigma stimulus-intervals, they were of such small amplitude that only a qualitative record could be made of their existence. For example, at 40 sigma, R_2 occurred only three times in the 300 trials recorded. The number of R_2's was greater at 80 sigma, and still greater at 160 sigma.

(2) Both R_1 and R_2 for each group undergo a progressive negative adaptation. This confirms previous findings as to the effect of repetition of stimulation on reflex response.

(3) Negative adaptation of R_1 at the 40 sigma and 80

[3] For a more extensive description of the apparatus the reader is referred to (2).

TABLE 1

AVERAGE AMPLITUDES (IN MM.) OF R_1 AND R_2 FOR ALL STIMULUS-INTERVALS (60 TRIALS)

	Trial	Zero	20σ	40σ	80σ	160σ	320σ	640σ	1 sec.	2 sec.	3 sec.	4 sec.
R_1	1–5	14.1	18.8	17.8	19.5	20.4	11.4	15.0	12.8	11.7	13.9	11.7
	6–10	9.7	14.7	18.8	17.8	12.0	7.5	11.3	8.6	4.4	9.1	6.9
	11–15	9.0	10.8	19.5	17.4	12.7	6.6	9.9	7.7	.8	9.4	9.8
	16–20	8.4	10.5	13.4	16.5	10.3	6.0	8.8	6.0	.6	7.3	10.4
	21–25	5.6	9.9	15.2	15.3	10.6	2.8	9.1	4.5	.2	6.9	9.8
	26–30	5.0	9.1	10.2	17.1	9.3	4.4	9.9	5.9	.3	3.6	9.7
	31–35	4.6	8.5	12.3	14.0	9.9	2.7	7.1	6.5	.1	3.2	7.6
	36–40	4.4	7.5	12.2	17.1	9.1	2.4	8.1	4.2	.0	2.2	8.4
	41–45	5.4	7.8	10.2	17.2	9.5	2.7	8.3	5.4	.3	3.9	11.7
	46–50	4.3	7.7	13.3	16.0	8.8	1.9	5.8	4.8	.0	4.0	12.0
	51–55	4.4	6.9	10.2	12.5	9.4	1.3	6.3	5.1	.0	1.7	10.8
	56–60	4.1	6.8	11.1	13.4	8.4	1.3	4.8	4.7	.0	2.1	10.1
R_2	1–5						1.8	8.4	3.6	3.0	10.7	11.4
	6–10						1.5	6.1	2.4	.4	7.1	9.3
	11–15						.5	3.9	2.5	.2	4.6	9.3
	16–20						1.7	4.9	1.9	.0	5.2	10.2
	21–25						.6	5.8	1.6	.0	1.9	9.4
	26–30						.6	3.7	2.4	.1	.7	10.0
	31–35						.3	3.4	2.8	.0	1.4	8.8
	36–40						.9	4.3	2.7	.0	.4	8.7
	41–45						.7	4.6	2.8	.2	.4	10.2
	46–50						.1	3.2	2.5	.0	.7	10.4
	51–55						.2	2.0	2.9	.0	.5	9.6
	56–60						.2	1.5	2.7	.0	.6	8.9

sigma stimulus-intervals is much more irregular than that at other stimulus-intervals.

(4) There is a differential amplitude decrement dependent, apparently, on stimulus-interval. Table 2 presents the

TABLE 2

TOTAL PERCENTAGE OF NEGATIVE ADAPTATION OF R_1 AND R_2 FOR ALL STIMULUS-INTERVALS (PERCENTAGE DIFFERENCE BETWEEN AMPLITUDE OF FIRST AND LAST FIVE TRIALS)

Stimulus-Interval	R_1	R_2
zero	70.9	
20 sigma	62.7	
40 sigma	37.7	
80 sigma	31.3	
160 sigma	58.8	
320 sigma	88.6	88.9
640 sigma	68.0	82.2
1 second	63.3	25.0
2 seconds	100.0	100.0
3 seconds	84.8	94.4
4 seconds	13.7	22.0

percentage differences between the average of the first five trials and the last five trials for each response.

The relative speed of negative adaptation for each stimulus-interval may, therefore, be summarized as follows: the greatest adaptation is undergone by the group at the stimulus-interval of 2 seconds, followed in order of lessening adaptation by the groups at 3 seconds, 320 sigma, zero, 640 sigma, 1 second, 20 sigma, 160 sigma, 40 sigma, 80 sigma, 4 seconds. Adaptation of R_2 is also greatest at 2 seconds, followed in order of decreasing adaptation by the groups at 3 seconds, 320 sigma, 640 sigma, 1 second, 4 seconds, demonstrating absolute agreement with differential adaptation of R_1. R_2's for the zero, 20 sigma, 40 sigma, 80 sigma, and 160 sigma stimulus-intervals are not presented because of lack of sufficient quantitative data.

Order seems to be brought out of the apparent chaos of the alignment of these series if the curve of differential adaptation (Fig. 1) is examined. This graph demonstrates the relationship between stimulus-interval and percentage of adaptation as given in Table 2. In this curve the following features of significance are suggested:

(1) Effectiveness of stimuli in producing negative adaptation is greater as the interval between stimuli is increased from zero to 80 sigma.

(2) The curve of negative adaptation follows a definite wave-like form with low points (resistance to adaptation) at 80 sigma, 640 sigma, 1 second, and 4 seconds; and high points (susceptibility to adaptation) at 320 sigma and 2 seconds. Intervening points make this curve a fairly smooth one: it will be noted that the periods of successive phases are increasingly greater.

(3) Negative adaptation of R_2 follows a wave-like curve fairly homologous with that of the R_1 curve.[4]

[4] It is interesting to note that the correlation coefficient (ρ) between negative adaptation of R_1 and R_2 is $+.943$; when converted to the product moment r it is $+.947$, with a P.E. of $\pm.029$. Of course, there are only 6 cases represented in these figures, but the close relationship is suggested nevertheless. This phenomenon, together with other data of a similar nature, will be discussed in a later paper.

Discussion

In a previous paper (2) the recovery of excitability during the refractory phase of the eyelid reflex was presented. It was seen that this curve was wave-like in form, *i.e.*, that the recovery of excitability was not in simple arithmetical progression, but rather was a complex rhythm of alternating recovery and depression.

The comparison of the wave-like curves of recovery of excitability of the three subjects in that investigation and an apparently homologous curve of adaptation of the fifty-five subjects of this investigation seems to be demanded. If the tables presenting the data of that investigation are examined, therefore, it will be seen that the mean average percentage amplitudes of R_2 (calculated from the R_2/R_1 ratio) are related to the total percentage of negative adaptation of response for the subjects of the present investigation in an inverse manner. The relevant portions of these tables are presented again for convenience.

Stimulus-Interval	Total Percentage of Negative Adaptation of R for Stimulus-Intervals of 320 sig.–4 sec.		Average Percentage of R_2 for 3 Subjects for Stimulus-Intervals of 320 sig.–4 sec.
	R_1	R_2	
320 sigma	88.6	88.9	33.8
640 sigma	68.0	82.2	55.1
1 second	63.3	25.0	72.7
2 seconds	100.	100.	50.0
3 seconds	84.8	94.4	62.9
4 seconds	13.7	22.0	86.2

It may be seen that although the direction of this inverse relationship between negative adaptation and excitability is consistent for these stimulus-intervals, absolute amounts of concomitant variation do not follow the above generalization. Thus, assuming the rough quantitative proportionality of the negative adaptation-excitability ratios at 320 sigma, 640 sigma, and 1 second to be correct, one would expect at the 2 second stimulus-interval either a smaller percentage of negative adaptation for the obtained percentage of excitability, or

the obverse, *i.e.*, a smaller percentage of negative adaptation. This same difficulty exists at the 3 second stimulus-interval. The 4 second stimulus-interval, however, preserves the quantitative aspect of the generalization. The differential negative adaptation of response seems to follow, therefore, a curve of wave-like form of a nature reciprocal with the homologous

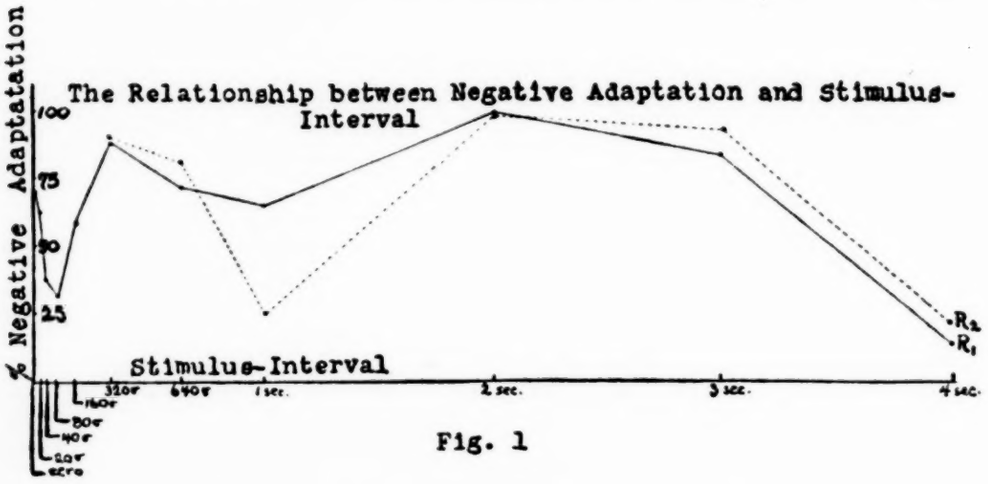

Fig. 1

curve of excitability. The correspondence of these two curves argues for the validity of the statement that one may be a function of the other.[5]

It was seen that the rate of negative adaptation was found to decrease as the interval between stimuli from zero to 80 sigma was increased. If the reciprocal relationship of excitability of R_2 and negative adaptation is true, these data would indicate a progressive increase of excitability as the intervals between stimuli are increased up to 80 sigma, despite the absence of recorded secondary response.

Why should this relationship between excitability of the reaction-system and its speed of negative adaptation be inverse? Our data demonstrate this, and there seem to be theoretical reasons also for its validity, namely, the refractory phase theory of inhibition and, by analogy, Heymans' Law of Inhibition.

Briefly, the refractory phase theory of inhibition states

[5] The correlation coefficient (ρ) between negative adaptation of R_1 and excitability was found to be $-.89$ ($r = -.90$, P.E. $= \pm.053$) and that between negative adaptation of R_2 and excitability to be $-.71$ ($r = -.73$, P.E. $= \pm.134$). Since n for both these figures is only 6, the values may have limited significance, but the mathematical relationship of the variables is, nevertheless, demonstrable.

that inhibition is set up by the falling of successive stimuli within the refractory phase of the stimuli which precede them. If the stimuli are given at a fast rate, total inhibition is achieved because of the absolute refractory phase; if the stimuli are presented at a slower rate, involving the relative refractory phase, various degrees of partial inhibition may be set up; if the stimuli are so presented that they fall within the rebound of previous stimuli, the effect is not inhibition, but summation. If one may consider negative adaptation as a type of protracted inhibition, it follows that stimuli spaced at intervals of low excitability will undergo greatest negative adaptation (inhibition) which will be less complete the greater the excitability of the reaction-system. Furthermore, stimulation in the supernormal phase, by nature of the summation of excitability set up, would be most resistant to modification in this form. Obviously, the relevance of these experiments to the refractory phase theory can be assured only by verifying the two assumptions implied; viz., that excitability as used in the present sense is truly a case of excitability or irritability as physiologically understood; and secondly, that negative adaptation is a case of protracted inhibition. If the above reasoning and its assumptions are correct, the present investigation points to the significance of the refractory phase in producing inhibition.

This inverse relationship between excitability and negative adaptation (inhibition) is not without precedent in the psychological literature. In investigating the validity of Heymans' Law, Spencer (8) found a correlation of +.907 between visual threshold and its susceptibility to inhibition for 50 subjects, and Spencer and Cohen (9) demonstrated a correlation of +.955 and +.968 for these same variables for 50 cases in a single subject, these coefficients running as high as +.992 in the last half of the experiment. In other words, both for the group and for the individual, thresholds were higher as susceptibility to inhibition was greater, increased thresholds, of course, meaning decreased excitability. Here the assumptions must be made that excitability of a reflex reaction-system at various moments within the relative refractory phase is

analogous to sensory threshold, and that negative adaptation is analogous to sensory inhibition. Since excitability (threshold) was found to be correlated to an almost perfect degree with susceptibility to inhibition in accordance with Heymans' Law, an analogous phenomenon in the present data would seem to fall within that generalization.

Conclusion

From these data it seems evident that the speed of negative adaptation of reflex response is a function of the degree of excitability of the reflex at given moments in the refractory phase. These two variables were found to be reciprocal, thus indicating a mathematical relationship between speed of extinction of reflex response and specific excitability of the reflex reaction-system. The validity of this inverse relationship is argued ex post facto from the refractory phase theory of inhibition and Heymans' Law.

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(Manuscript received June 23, 1931)

THE EFFECT OF REFRACTORY PHASE UPON NEGATIVE ADAPTATION OF PRIMARY REFLEX RESPONSE

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In two earlier papers in which the relationship of negative adaptation of reflex response and the refractory phase of the reflex was investigated (1, 2), it was experimentally demonstrated that the refractory phase was related to the speed of negative adaptation of both the primary and the secondary responses. This phenomenon was pointed out to be quite peculiar in the light of the fact that only R_2 (reaction to the second stimulus) was evoked during refractory phase. A discussion of this phenomenon, with particular reference to its theoretical significance, is presented in this paper.

The speed of negative adaptation of the body-start reflex of the guinea pig to sudden noise was shown to be related to refractory phase, R_1 and R_2 following homologous curves of diminution of response (1). These data were procured from 4 groups of 2 subjects, each of which underwent 200 trials. Each trial consisted of 2 noise stimuli. For each of the 4 groups these noise stimuli were spaced 1, 2, 3, and 4 seconds apart respectively, but for all subjects successive trials were spaced 30 seconds apart. Thus, although the time interval between S_1 and S_2 varied between subjects, the time interval between S_2 and the next S_1 was always 30 seconds.

Similarly for the lid-reflex in human subjects was this same phenomenon observed (2). In that investigation 55 human subjects were separated into 11 groups of 5 each. All subjects underwent 60 trials. Between S_1 and S_2 there was allowed a time interval of zero, 20 sigma, 40 sigma, 80 sigma, 160 sigma, 320 sigma, 640 sigma, 1 second, 2 seconds, 3 seconds, 4 seconds, respectively, but suitable quantitative data concerning R_2 were procured for the latter 6 groups only.

Here, also, the time interval between S_2 and the next S_1 was constantly 30 seconds for all subjects, but despite this, R_1 followed a curve of negative adaptation homologous with that of R_2. It was pointed out in that investigation that the correlation (ρ) between negative adaptation of R_1 and that of R_2 was $+.942$ ($r = +.947$, P.E. $= \pm.029$), but the fact that n was only 6 may limit the significance of this value.

Why this apparently 'retroactive' effect should occur is not immediately obvious. For all the subjects at the various stimulus-intervals investigated in both experiments the first responses per se were initially evoked under apparently identical conditions, viz., 30 seconds after the preceding S_2. The fact that the relative rates of negative adaptation of *both* responses followed the same relative courses leads one to believe that the diminution of primary reflex response was due to the existence of some factor operating during the 30 second interval between the second stimulus of one pair and the first stimulus of a succeeding pair. It is difficult to hypothesize with certainty that the excitability of R_2 is the cause of the negative adaptation of R_1, but the writer is inclined to believe that this is the case, nevertheless.

The argument in support of this statement may be advanced by the use of abstract symbols in this manner: let x_1 represent stimulus-interval; let x_2 represent negative adaptation of R_2; let x_3 represent negative adaptation of R_1. If r_{23} is say .80, and r_{12} is say .80, r_{13}, because of this, is some significant figure, which may be as high as .64. This is not because of the relationship between x_1 and x_3, but because of their common relationship with x_2. It may well be true, therefore, that the only reason for the apparent relationship of stimulus-interval and negative adaptation of R_1 is the fact that R_1 and R_2 occur in a continuous series and that the effect of stimulus-interval on R_2 is transmitted to R_1 similarly to its influence on R_2.[1]

[1] An analogy of a somewhat similar situation is the classical smallpox correlation case. The proposition is: to prove that wealth, which has nothing to do with the incidence of smallpox, may be related with incidence of smallpox as high as .64 if both are involved in a third variable, *e.g.*, inoculation.

Summary and Conclusion

Experimental evidence has been presented of the relationship of negative adaptation and refractory phase of reflex response. It was shown that the influence of refractory phase was such as to determine not only the speed of negative adaptation of responses occurring during its incidence, but that a similar effect was produced upon responses which occurred before its onset. All these primary responses were evoked at constant intervals after preceding trials, but despite this, their negative adaptation seemed to depend upon the time interval of refractory phase which was to elapse until a second stimulus evoked another response. It is suggested that this phenomenon may be due to the transmission of the effect of specific excitability of responses occurring within the refractory phase in such a way as to determine the specific modification of responses evoked by stimuli presented after a long, constant time interval, and after the termination of refractory phase. It is possible that it is the constancy of the length of this time interval which makes such responses subject to this effect. Or, it may be that specific excitability, tapped at a given moment during refractory phase, is capable of perseveration, so as to determine the effectiveness of subsequent stimuli in producing modification of response.

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Before inoculation against smallpox was introduced, individuals at all levels of wealth suffered equally. If we let x_1 represent the incidence of smallpox, x_2 represent inoculation, and x_3 represent wealth, we can express this by saying that $r_{13.2} = 0$. With the introduction of inoculation treatment, however, the simple relationship between wealth and the incidence of smallpox rises from 0, and may take an apparently significant value, *e.g.*, .64, for if the wealthier are more likely to be inoculated, and if the inoculated are less susceptible to smallpox so that the correlation between wealth and inoculation is +.80, and that between inoculation and smallpox is −.80, r_{13} in the formula

$$r_{13.2} = \frac{r_{13} - r_{12}r_{23}}{\sqrt{1 - r_{12}^2}\sqrt{1 - r_{23}^2}}$$

may have the value of −.64 when $r_{13.2} = 0$.

(Manuscript received June 23, 1931)

CORRELATION BY CALCULATING-MACHINE

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A recent bulletin from the University of Nebraska describes in detail the mechanics of A Machine Method of Calculating the Pearson Correlation Coefficient.[1] This method is basically identical with that used in our Bureau of Personnel Research at the University of Oregon, and the following account of our method is given for three reasons: (1) to furnish more insight into the significance of the machine manipulations; (2) to correct a few mistaken impressions which might result from the Nebraska bulletin; and (3) to suggest one or two improvements on the method which we have found useful.

The correlation between two variables or two sets of measurements or "scores" is the "concomitant variation" of the scores. If a high value of variable X is more often than not —throughout the measurements made—associated with a high value of variable Y there is positive correlation between X and Y, and the correlation coefficient r_{XY} has a value somewhere between 0 and + 1. If high values of X are more often than not associated with low values of Y, the correlation is negative and r_{XY} lies somewhere between − 1 and 0. The value of r may be found from any one of many equivalent variations of the Pearson formula.[2] Our method uses raw scores, reduced to two digits for convenience; thus scores of 321 and of 177 would become 32 and 18, respectively. (Such reductions do not affect the value of r_{XY},[3] but must be kept in mind for any subsequent interpolation of means and standard deviations.) Accuracy is satisfactory with but two digits, as long as this provides a range of about twenty score intervals.[4]

[1] By E. E. Tremmel and C. C. Weidemann, 1930, 15 pp., 35 cents; the University of Nebraska Extension Division, Lincoln, Neb.

[2] See Variations of the Product-Moment (Pearson) Coefficient of Correlation, by P. M. Symonds, *Jour. of Educ. Psy.*, 1926, 17, 458–469.

[3] Statistical Method, by T. L. Kelley, 1924, Macmillan, p. 198.

[4] Kelley, p. 52. Statistical Methods, by F. C. Mills, 1924, Henry Holt, p. 69.

Either of two formulas for r_{XY} is recommended for this method. The first uses directly the summations made on the calculating-machine; it is the faster of the two. The second requires the computation of means (M) and of standard deviations (σ) previous to finding r_{XY}; since these values are needed anyway for a thoroughgoing analysis of correlation, this formula is usually preferable.

$$(1) \qquad r_{XY} = \frac{N\Sigma XY - \Sigma X \Sigma Y}{\sqrt{[N\Sigma X^2 - (\Sigma X)^2][N\Sigma Y^2 - (\Sigma Y)^2]}},$$

$$(2) \qquad r_{XY} = \frac{\frac{\Sigma XY}{N} - M_X M_Y}{\sigma_X \sigma_Y},$$

where

$$M_X = \frac{\Sigma X}{N}, \qquad M_Y = \frac{\Sigma Y}{N};$$

$$\sigma_X = \sqrt{\frac{\Sigma X^2}{N} - M_X^2}, \qquad \sigma_Y = \sqrt{\frac{\Sigma Y^2}{N} - M_Y^2}.$$

There are two quite distinct steps in obtaining r_{XY}: (1) summing the raw scores to find ΣX, ΣY, ΣX^2, ΣY^2, ΣXY, and N; (2) substituting these summations in the chosen formula and solving for r_{XY}. It is the former which is greatly improved by this calculating-machine method as compared with the labor required if the scores are plotted on paper.

The Nebraska bulletin states: "This method requires the use of the Monroe Adding-Calculator. . . ." That is incorrect; the mechanical details as therein listed do require that particular machine, but the method is not so restricted. Our experience is that the ideal machine for this method is the Marchant 'full automatic' (model EEG-9); this has a nine-column keyboard and eighteen-column lower dial, electric operation including automatic multiplication and division, adjustable automatic carriage shift, convenient dial clearance. The fast, smooth carriage shift is its main advantage for this method of computing correlations; with less speed, any calculating-machine may be used which has continuously cumulating dials. The following brief demonstration of the method is adapted primarily to the Marchant.

Summation

Now for a demonstration of the method let us assume we have data of which a small fragment is:

Case	Score X	Score Y
A	2	9
B	15	34
C	8	20
etc.		

Set the decimal markers on the machine at 0, 6, and 12, counting from the right; this most fully utilizes the dial space. Thus on the keyboard there are the right sector including rows 1 to 6, and the left sector from rows 7 to 9; on the upper dials there are the same; on the lower dial there are the right sector from rows 1 to 6, the middle from rows 7 to 12, the left from rows 13 to 18. The left is the X sector, the Y the right; as we shall see, the middle is for the cross-product XY.

Now set the scores for case A on the keyboard:

002,000009,

Shift the carriage so that row 13 on the lower dial is under row 7 on the upper left dial, that is, so that the *left* (X) *sectors* of these dials *are in line* one above the other. (On the Monroe, set row 7 on the carriage over row 1 on the keyboard.) Multiply by 2 (which is the X score set on the keyboard).

Now the upper right dial reads: 002,000000,

This is X. And the lower dial reads: 000004,000018,000000,

This is X^2 in the left sector and XY in the middle sector. (We have multiplied by X (= 2) the keyboard settings of X (= 2) and Y (= 9).)

Shift the carriage to *line up the right* (Y) *sectors* one above the other, bringing row 1 on the lower dial under row 1 of the upper left dial. (On the Monroe, set row 1 on the carriage over row 1 on the keyboard.) Multiply by 9 (that is, by Y).

Now the upper right dial reads: 002,000009,

And the lower dial reads: 000004,000036,000081,

The upper right dial now contains X and Y. The lower dial contains our previous products X^2 and XY, plus YX in the middle sector and Y^2 in the right sector. (This time we have multiplied by Y (= 9) the keyboard settings of X (= 2) and Y (= 9).) Thus the lower dial, from left to right, reads (X^2 =) 4, ($XY + YX = 2XY$ =) 36, (Y^2 =) 81.

Do not clear dials until all cases have been entered and the cumulative totals copied down.

In similar fashion for case B, set 15 in rows 7 and 8 and set 34 in rows 1 and 2: 015,000034,

With row 1 of the lower dial under row 1 of the upper left dial, multiply by 4; shift the carriage one row to the right, and multiply by 3. Shift so that row 13 of the lower dial is under row 7 of the upper left dial, and multiply by 5; shift the carriage one row to the right, and multiply by 1. Similarly multiply in the scores for case C.

The cumulations for cases A, B, C are read from the dials as follows:

Upper right dial: 025,000063,

thus $\Sigma X = 25$, $\Sigma Y = 63$.

Lower dial: 000293,001376,001637,

thus $\Sigma X^2 = 293$, $2\Sigma XY = 1376$, $\Sigma Y^2 = 1637$.

The whole process may be expressed algebraically in this way:

$$(X + Y) \times (X + Y) = X^2 + 2XY + Y^2.$$

set on keyboard	multiplied into upper dial	resulting on lower dial

Summing, changing the keyboard setting for each case in succession but cumulating in the dials, yields the required ΣX and ΣY, ΣX^2 and ΣY^2, ΣXY (from $2\Sigma XY \div 2$). The number of cases, N, must be counted separately.

We may introduce a check on the accuracy of the summations by a modification of Charlier's checking method.[1] Algebraically,

$$\Sigma(X + 1)(Y + 1) = \Sigma XY + \Sigma X + \Sigma Y + N \qquad (N = \Sigma 1).$$

[1] See Mills, p. 157.

Hence if we cumulate the products of (each X score plus 1) by (each Y score plus 1), we should be able to check that against the sum of $(\Sigma XY + \Sigma X + \Sigma Y + N)$. Referring to our demonstration data for cases *A*, *B*, *C:*

$$(2 + 1) \times (9 + 1) + (15 + 1) \times (34 + 1) + (8 + 1) \times (20 + 1) = 779,$$

$$\Sigma XY + \Sigma X + \Sigma Y + N = 688 + 25 + 63 + 3 = 779;$$

therefore the summations are correct. Only ΣX^2 and ΣY^2 are left unchecked, and the accuracy of ΣX, ΣY, and ΣXY practically insures the accuracy of the sums of the squared terms. (Exactly compensating errors in the keyboard settings of X and Y would be required for this to fail, and such coincidence is very improbable.)

Our speed on the Marchant is about the same as that reported, in the Nebraska bulletin, on the Monroe. To obtain and record summations, we required 9 minutes for each of 15 correlations of 60 cases each, and $7\frac{1}{2}$ minutes for each of 465 correlations averaging 45 cases each. The Charlier check takes somewhat less time, depending of course on the number of errors found and requiring correction; in our experience, the extra work of checking is likely to be well justified by increased accuracy of results.

Substitution and Solution

If the No. 1 formula for r_{XY} is to be used the computation is quite straightforward. We would suggest that the denominator first be evaluated and that the following routine of operations is economical:

Find $(\Sigma X)^2$, note down on paper, clear machine.

Find $N\Sigma X^2$, leave in machine. Subtract $(\Sigma X)^2$, note down remainder, clear.

Similarly find $(\Sigma Y)^2$; find $N\Sigma Y^2$ and subtract $(\Sigma Y)^2$, note down remainder, clear machine.

Multiply these two remainders, leave in machine. Extract square root of their product, giving *denominator* of formula.

Find $\Sigma X \Sigma Y$, note down, clear machine.

Find $N\Sigma XY$, leave in machine. Subtract $\Sigma X\Sigma Y$ (giving *numerator*), leave in machine; clear upper dial only.

Divide by denominator, giving r_{XY}.

If the No. 2 formula for r_{XY} is to be used, the following form is suggested as one we have found convenient and simple to carry through:

	N		
ΣXY	$\frac{\Sigma XY}{N}$		r_{XY}
ΣX	M_X	M_X^2	
ΣY	M_Y	M_Y^2	M_XM_Y
ΣX^2	$\frac{\Sigma X^2}{N}$	σ_X	
ΣY^2	$\frac{\Sigma Y^2}{N}$	σ_Y	$\sigma_X\sigma_Y$
(Sums)	(Sums $\div N$)		

Simple division by N produces the second column from the first. The third is a mixture provided to aid with the formula $\sigma_X = \sqrt{\Sigma X^2/N - M_X^2}$. The products of the means and of the standard deviations, respectively, are entered in the fourth column. Then r_{XY} is obtained by putting $\Sigma XY/N$ in the lower dial, subtracting M_XM_Y, and dividing by $\sigma_X\sigma_Y$.

Whichever formula for r_{XY} is used for the solution, the other may be utilized as a check.

Fifteen correlations, of 60 cases each, required an average of 5 minutes each for computation after the summations were made, by formula No. 1. By formula No. 2, more than 800 correlations with an average of 45 cases each required $4\frac{1}{2}$ minutes apiece after the summations. Thus our time for completely computing Pearson product-moment correlation coefficients—from original data to r_{XY}—was less than 13 minutes each for groups of about 50 cases.

We would estimate that complete, checked solution for r_{XY} by this method would require about 25 minutes for a population of 50 cases, about 40 minutes for 100 cases. This allows for the Charlier check on the summations, and for the use of both formulas for r_{XY}, one to check the other. Thus

there is an absolute guarantee of accuracy for all the steps from the recorded original data to the recorded final results taken from the calculating-machine dials. Correlation procedures in which the scores are plotted on charts are never so fully checked—unless entirely repeated—because there is no check on the accuracy of plotting. The machine method is of great advantage in handling intercorrelations, because when the mean and standard deviation have once been determined for each variable only the cross-products need be computed for each pair of the variables; if plotted, each intercorrelation must be carried through the entire computation.

We recommend this calculating-machine method of computing the Pearson correlation coefficient on the grounds of speed and accuracy. Disadvantages are that it requires an expensive machine for greatest efficiency, and that, lacking a plot of the scores, there is no way to determine linearity of regression. The method is of particular value when a number of rather routine correlations are sought.

(Manuscript received May 18, 1931)

COMPARABLE SCORES FROM SKEWED DISTRIBUTIONS

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In another article[1] the writer has indicated a method for converting skewed distributions into distributions which are approximately normal, having a mean of zero and a standard deviation of unity. The advantage of this procedure is that it offers a perfectly straightforward method for obtaining comparable scores from distributions which are wholly dissimilar in shape. The method, however, is available only when the number of cases is large enough to present rather definitely graduated distribution curves.

The procedure involves a well-known mathematical operation known as a transformation of variable. If the original scores are indicated by X we assume that these may be transformed to new scores Z where

$$Z = f(X) = a_0 + a_1X + a_2X^2 + \cdots . \qquad (1)$$

Here we have a simple power series in X where the coefficients of the successive powers are determined from the data. In the article previously referred to, it is assumed that the transformation can be satisfactorily made by dropping all but the first four powers of the series. While the computations required for determining the coefficients are not involved, they may require more labor than those not familiar with statistical calculations might wish to expend. Often the distributions may be "normalized" to a sufficient degree of accuracy by retaining merely the first two powers of the series, thus

$$Z = a_0 + a_1X + a_2X^2. \qquad (2)$$

By determining the constants in the manner to be described, it is possible to obtain approximately normal distri-

[1] Journal of the American Statistical Association, Dec. 1931, p. 455.

butions of Z scores which are comparable to 'standard' scores of the form

$$Z = \frac{X - M}{\sigma}, \tag{3}$$

with the additional advantage that they are distributed equally on both sides of the mode. To give a fairly close approximation to the normal curve, however, the original distribution should be neither unusually flat nor unusually peaked about the node.

The method is as follows: First the range of raw scores is determined and this range divided equally into an even number of class intervals. About 8 or 10 class intervals is adequate. A distribution plot is then made to make sure that the scores present a comparatively smoothly graduated distribution. The percentage of cases included in the first $n/2$ of the class intervals is determined, where n is the total number of class intervals. From a table of the normal probability integral the sigma value of this percentage is found. Let this value be indicated by K and given the positive or negative sign according as the percentage is greater or less than .5 of the total number of cases.

Since there are three constants in (2) to be determined, we must have three equations for three distinct values of X. We assume that the approximation is close enough if we consider only the part of the normal curve between -3 and $+3$. We take the origin of the distribution of raw scores at the upper limit of the $n/2$ class interval and indicate the upper and lower limits of the distribution by $+1$ and -1 respectively.

Then we obtain the three equations

$$\begin{aligned} -3 &= a_0 - a_1 + a_2, \\ K &= a_0, \\ 3 &= a_0 + a_1 + a_2, \end{aligned} \tag{4}$$

or solving (4) for the a's we get simply

$$\begin{aligned} a_0 &= K, \\ a_1 &= 3, \\ a_2 &= -K. \end{aligned} \tag{5}$$

Substituting the values of the a's given in (5) in (2) we have

$$Z = K + 3X - KX^2. \tag{6}$$

But the X's in (6) are taken in such units as to range from only -1 to $+1$, and are measured from zero as an origin. If we let A be the raw score at the upper limit of the $n/2$ class interval and B equal ½ the range of raw scores, we may write

$$x = \frac{X - A}{B}. \tag{7}$$

Substituting (7) in (6) we get

$$Z = b_0 + b_1 x + b_2 x^2, \tag{8}$$

where

$$\begin{aligned} b_0 &= K - 3\left(\frac{A}{B}\right) - K\left(\frac{A}{B}\right)^2, \\ b_1 &= \frac{3}{B} + \frac{2KA}{B^2}, \\ b_2 &= \frac{-K}{B^2}. \end{aligned} \tag{9}$$

The procedure is considerably less laborious than solving for the means and sigmas required for obtaining standard scores. All the constants are obtained from equations (9) and these involve only the experimental constants K, A, and B which are gotten at once from the data.

If it is desired to convert all the raw scores to standard "normal" scores, it is best to make up a transmutation table in the following manner. In column (1) write the consecutive integers included between the lowest and highest scores in the distribution. In column (2) copy the square of these integers consecutively from a table of squares. Column (3) is column (1) multiplied by b_1. Column (4) is column (2) multiplied by b_2. (5) is the algebraic sum of columns (3) and (4) plus b_2, and gives the converted scores.

For most of the work done with test scores in psychology and education, this procedure gives a sufficiently accurate and very simple transmutation.

If from previous or current experience with the data to be used one is justified in assuming a comparatively smooth distribution, it is not even necessary to make a distribution plot of the scores. The range and its mid-point is found and the proportion of cases below the mid-point determined. From this proportion K is obtained. Since A and B are already given the constants for (8) may be solved for at once.

(Manuscript received May 5, 1931)

THE DIFFICULTY OF MULTIPLE CHOICE TEST ITEM ALTERNATIVES

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By the difficulty of a multiple choice test item alternative we mean the amount of discriminatory ability required to differentiate between the idea expressed in the text of the item and the idea expressed in the alternative. Thus, for a multiple choice item which has n alternatives, were present the discriminatory difficulty of each by $x_1, x_2, \cdots x_n$.

Now suppose we have a group of individuals whose distribution of ability to discriminate with reference to the test item under consideration takes the form $f(x)$. We arrange the alternatives in order of difficulty so that

$$x_1 < x_2 < \cdots < x_n,$$

and we indicate by $S_1, S_2, \cdots S_n$ the percentage of individuals in the group which mark the respective test items. The proportion of individuals possessing an amount of discriminatory ability included between the difficulty of any two alternatives X_i and X_j may be indicated by

$$\int_{x_i}^{x_j} f(x)dx.$$

It is shown in another article [1] that the proportion of individuals marking each alternative is composed of the following proportions of various degrees of discriminatory ability,

[1] Article to be published in the Journal of Educational Psychology.

$$\left.\begin{aligned}
S_1 &= \frac{1}{n}\int_{x_0}^{x_1} f(x)dx, \\
S_2 &= \frac{1}{n}\int_{x_0}^{x_1} f(x)dx + \frac{1}{n-1}\int_{x_1}^{x_2} f(x)dx, \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
S_{n-1} &= \frac{1}{n}\int_{x_0}^{x_1} f(x)dx + \frac{1}{n-1}\int_{x_1}^{x_2} f(x)dx + \cdots + \frac{1}{2}\int_{x_{n-2}}^{x_{n-1}} f(x)dx, \\
S_n &= \frac{1}{n}\int_{x_0}^{x_1} f(x)dx + \frac{1}{n-1}\int_{x_1}^{x_2} f(x)dx + \cdots \frac{1}{2}\int_{x_{n-2}}^{x_{n-1}} f(x)dx \\
&\qquad + \int_{x_{n-1}}^{x} f(x)dx,
\end{aligned}\right\} (1)$$

where x_0 in the lower limit of the first integral of each equation and x in the upper limit of the last integral of the last equation are the lower and upper roots respectively of $f(x)$.

If now we subtract from each equation the one preceding it, we get

$$\left.\begin{aligned}
S_1 &= \frac{1}{n}\int_{x_0}^{x_1} f(x)dx, \\
S_2 - S_1 &= \frac{1}{n-1}\int_{x_1}^{x_2} f(x)dx, \\
S_3 - S_2 &= \frac{1}{n-2}\int_{x_2}^{x_3} f(x)dx, \\
&\cdots\cdots\cdots\cdots\cdots \\
S_{n-1} - S_{n-2} &= \frac{1}{2}\int_{x_{n-2}}^{x_{n-1}} f(x)dx, \\
S_n - S_{n-1} &= \int_{x_{n-1}}^{x} f(x)dx,
\end{aligned}\right\} (2)$$

or dividing each equation by the coefficient of the integral term and transposing we get

$$\left.\begin{aligned}
\int_{x_0}^{x_1} f(x)dx &= nS_1, \\
\int_{x_1}^{x_2} f(x)dx &= n-1\,(S_2 - S_1), \\
\int_{x_2}^{x_3} f(x)dx &= n-2\,(S_3 - S_2), \\
&\cdots\cdots\cdots\cdots\cdots \\
\int_{x_{n-2}}^{x_{n-1}} f(x)dx &= 2(S_{n-1} - S_{n-2}), \\
\int_{x_{n-1}}^{x} f(x)dx &= (S_n - S_n = 1).
\end{aligned}\right\} (3)$$

Equations (3) give the proportions of individuals of discriminatory ability included between the difficulty of any two successive alternatives.

Suppose next we add equations (3) cumulatively from the top down. Since, in general

$$\int_{x_i}^{x_{i+1}} f(x)dx + \int_{x_{i+1}}^{x_{i+2}} f(x)dx = \int_{x_i}^{x_{i+2}} f(x)dx, \tag{4}$$

this cumulation gives

$$\left.\begin{aligned}
\int_{x_0}^{x_1} f(x)dx &= nS_1, \\
\int_{x_0}^{x_2} f(x)dx &= nS_1 + n-1\,(S_2-S_1), \\
\int_{x_0}^{x_3} f(x)dx &= nS_1 + n-1\,(S_2-S_1) + n-2\,(S_3-S_2), \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
\int_{x_0}^{x_{n-1}} f(x)dx &= nS_1 + n-1\,(S_2-S_1) + n-2\,(S_3-S_2)\cdots \\
&\qquad +(S_{n-1}-S_{n-2}),
\end{aligned}\right\} \tag{5}$$

or in general,

$$\int_{x_0}^{x_i} f(x)dx = \sum_{1}^{i} (n+1-i)\,(S_i - S_{i-1}). \tag{6}$$

If we define

$$F(x) = \int f(x)dx, \tag{7}$$

(6) may be written simply

$$F(x_i) = \sum_{1}^{i} (n+1-i)\,(S_i - S_{(i-1)}), \tag{8}$$

for by definition $F(x_0) = 0$.

From equation (8) we may calculate the difficulty of each alternative providing $f(x)$ is given. If we define our unit of measurement of discriminatory ability so that the distribution is normal and the standard deviation unity, and if we take as the origin the mean of the group, equation (6) may be written

$$\frac{1}{\sqrt{2\pi}} \int_{-\infty}^{x_i} e^{-x^2/2}\,dx = \sum_{1}^{i} (n+1-i)\,(S_i - S_{i-1}). \tag{9}$$

Since the right hand side of (9) is a numerical value x_i the difficulty of the i'th alternative can be readily determined from a table of the normal probability integral.

It should be noted that in none of the foregoing equations does x_n enter into the limits of the definite integrals. This means that the difficulty of the test item is determined by the difficulty of the $(n - 1)$'th alternative rather than the difficulty of the n'th alternative or best answer.

In particular, from (8) it is evident that when $S_1 = S_2 = \cdots = S_{n-1}$ then $x_1 = x_2 = \cdots = x_{n-1}$.

In this case the difficulty of the test item is simply the value of x in

$$\int_{x_0}^{x} f(x)dx = \frac{n}{n-1}(1 - A), \tag{10}$$

where A is the proportion marking the best answer. If there are only two alternatives and the unit of measurement is defined so that $f(x)$ becomes the normal probability function then (10) becomes simply

$$\frac{1}{\sqrt{2\pi}}\int_{-\infty}^{x} e^{-x^2/2}\,dx = 2(1 - A). \tag{11}$$

This means that the difficulty of the test item measured from the mean of the group in units of its standard deviation is the sigma value of twice the proportion of individuals marking the wrong alternative.

(Manuscript received May 13, 1931)